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INTROSPECTION AS AN OBJECTIVE METHOD¹

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The two most radical doctrines put forward by the behaviorist are, first, that consciousness does not exist, and second, that psychology should wholly abandon the introspective method. My chief object this evening is to inquire in what degree the second thesis necessarily follows from the first. But before entering on a discussion of the value of introspection to a behaviorist, it seems desirable that I should state, as briefly as possible, my attitude towards the first thesis, namely, that consciousness does not exist.

I was trained as a structural psychologist. Now it is an odd circumstance that while historically behaviorism in America is the child of functional psychology, logically the point of view of structural psychology leads more naturally to sympathy with behaviorism than does that of functional psychology. Structural psychology and behaviorism have the tremendous bond of accepting the world of physical science as a closed system. Structural psychology in addition to the world of physical science accepts a parallel or epiphenomenal world of consciousness; behavioristic metaphysics rejects this conscious concomitant. But both behaviorism and structural psychology refuse to allow the interference of non-physical causes in the physical system.

As a structural psychologist, then, I am prepared like the behaviorist to defend the unity of the physical world. It is

¹ Address of the president, before the American Psychological Association, Princeton Meeting, December, 1921.

a world of motions, the sum of which is constant. The signal triumph of this conception is that it is the only hypothesis ever framed by man upon which predictions may be made that will infallibly be verified. On no other hypothesis can man command obedience from his environment. Functional psychology, like much recent philosophizing, violates this closed system of science in the interests of monism: I would rather sacrifice monism than the physical order. In one of Dr. Kantor's ably written articles,¹ 'Psychology as a Science of Critical Evaluation,' the author, discussing memory, rejects the structuralist interpretation because it involves 'an empty dance of idea-atoms,' and behaviorism because it involves 'what is just as bad, a neural mechanics.' If we ask what Dr. Kantor would substitute, he answers that 'when we consider the function of ideas in memory, we must immediately freight those ideas heavily with human content.' My difficulty with this statement is just that I do not know what it means. A neural mechanics can be linked to the mechanistic system of the universe, which seems to me to have, by virtue of its success in controlling phenomena, so compelling a prestige; sensations I can fit into a relation of concomitance with the neural mechanics, but freights of human content are anomalies unless reduced to other terms. They seem to be of the same stuff as that which Münsterberg used to call 'will-attitudes,' and these, as he used earnestly to maintain, have no place as such in the world of science.

The structural psychologist feels, then, more at home in the behavioristic world of physico-chemically determined behavior than he does in a world where movements are causally determined by freights of human content, or by active selves. How did it happen, then, that functional psychology and not structural psychology was historically the parent of behaviorism? Why, the behaviorist learned from the functional psychologist to ignore the mind-body problem; he learned that in a world where ideas can act as such directly on movements there is no difference between ideas and the physical world of movements; hence he felt at liberty to take

¹ *PSYCHOL. REV.*, 1919, 26, 12-13.

a further step and say that ideas are movements and nothing else. By simple assertions the old concepts were redefined. Consciousness does not accompany a certain degree of complexity among systems of muscular movements: it *is* a certain degree of complexity of movement. What an animal means *is* what he does; thinking *is* subvocal talking. This fervor of assertion unaccompanied by argument, which recalls the methods of Mrs. Eddy, certainly points to the sincerity of the behaviorist's conviction that talking and thinking are one.

Believing with the behaviorist that the system of physical nature is a closed system, and envying as we all must the triumphal march of physical science, the structural psychologist must sympathize with the behaviorist's desire to attach himself to the mechanistic procession. There was at a certain university where I had charge of the psychological laboratory an intelligent janitor who had picked up many bits of scientific information by serving the various laboratories of the institution. While waiting in the laboratory of psychology one day he gazed meditatively about, and remarked: "This is a queer place. It somehow gives you the impression that the thing can't be done." Now the experimental work of the behaviorist has the great advantage that it plainly can be done; the untrained and casual observer can behold it being done. Upon this gratifying obviousness of his results the behaviorist is to be congratulated. The scientific worker who does not come into the world of physical science and observe merely movements is bound to be lonely and to miss the comradeship of the physicists and chemists.

But though we are sympathetic with the good fortune of the behaviorist in obtaining admission to the world of physical science, the rest of us cannot accept his statement that nothing but the world of physical science exists. For the world of physical science leaves out colors, tones, odors; the whole system of sensation qualities. A system of movements, it admits, ultimately, no qualitative differences in the bodies which do the moving. Solar systems, living organisms, molecules, atoms, are only different patterns or arrangements

in space: neither in the stimulus acting upon the organism, nor in the organism itself with its responses to stimulation, which are of course movements, do we get anything but rearranged patterns of qualitatively uniform particles. In reviewing Watson's first book, in 1915,¹ I said: "Once allow that a human being can experience a sensation of color or of taste or of anything except his own movements, and you have admitted the existence of a phenomenon not to be explained in behavioristic terms." It seems singular that behaviorists should overlook the fact that their world has no sensation qualities; they seem as it were to play at hide and seek with them. When it is a question of the reaction of the organism to blue light and they are thinking about the stimulus side of the stimulus-response situation, they accept the physicist's account of the stimulus as a series of ether disturbances and tacitly assume that the blueness is in the organism's response: when they are thinking about the response side of the situation, they accept the physiologist's account of it as molecular readjustments in nerve and muscle, and tacitly assume that the blueness was in the stimulus. Obviously, however, the blueness is neither in the stimulus nor in the response, for these are movements and blueness is not a movement. There is no escape, if the behaviorist wishes to accept the physical scientist's account of stimulus and response, from the recognition of something over and above these and of different stuff, namely, colors, tones, and the other sensation qualities; namely, consciousness,—abhorred word!

Annoying as it is to have to admit that we cannot eliminate dualism from our conceptions of the world, it can be shown, on the behaviorist's own theory of perception, that such a dualism arises from the very nature of the relations between the organism and its environment. The forces to which the organism reacts are of two kinds, molecular movements and mass movements. Where the stimulus is a mass movement, the movement of the organism in response is comparable to the movement of the stimulus. Now it would be good behaviorism to say that such stimuli can be adequately perceived because they can be adequately responded to: our

¹ *Phil. Rev.*, 1915, 24, 212.

movements in response to the stimulus are like those of the stimulus. The successive blows of raindrops on the hand are perceived with accuracy as distinct, discontinuous impulses, for the same rate and extent of movement is possible to the hand itself. It is obvious that all the properties which have been ascribed to stimuli, that is, to the physical world, whether by modern science or by the philosophers who wrote of the distinction between primary and secondary properties of matter, are derived from our own experience of our own movements; they are kinaesthetic. Locke said that the bulk, figure, number, situation, motion, and rest of their solid parts are in bodies whether we perceive them or not, and a modern physicist would describe matter in the same terms; now, bulk, figure, number, situation, motion, and rest are all experienced through our own movements. But when the hand is subjected to the action of heat, we perceive, not the dance of molecules striking against it, but heat, a continuous sensation quality, something quite different from any movement whatever, and this inaccurate and inadequate perception rests on the fact that we cannot make movements in response to the movements of these separate particles. We substitute a difference in the quality of something continuous for differences in the frequency of what is intermittent, because we are infinitely too large to make discontinuous movements corresponding to the discontinuous movements of molecules, atoms, or electrons. Thus there comes into existence something that has no resemblance to movement: colors, tones, odors, in a word, sensation qualities or consciousness. The only way to avoid recognizing this dualism is to abandon the idea of the physico-chemical nature of stimulus and response; to say either that movement is not the essential nature of the stimulus, or that the reaction of the organism is something other than movement. This latter alternative seems to be the one chosen by philosophical behaviorists like Professor R. B. Perry: when they say that consciousness is a form of behavior, they mean that behavior is something other than movement. But if you held that behavior is anything other than movement you have got to show what relation it has to

movement: you give up the whole physico-chemical theory of behavior, and behaviorism becomes as far out of harmony with all the rest of the sciences as the most cloudy mysticism could possibly be.

The metaphysics of behaviorism, therefore, I cannot accept. But it is not the main purpose of this address to attack a metaphysical problem. I should like, rather, to do something to further the harmonious working of behaviorists and introspective psychologists in company, by persuading the former that there is a point of view from which introspective results are important to behaviorism. It seems to me a deeply discouraging phenomenon, and one discreditable to psychology, that a great body of experimental data obtained through the expenditure of as much care and patience as have in other sciences resulted in discoveries of enduring worth, should be thrown aside by one school of psychologists as wholly valueless. May one who accepts—as I do not—the thesis that mental states are not a proper subject of scientific investigation, that they do not even exist, still extract trustworthy and important results from introspection? In order to answer this question, I shall try throughout to assume the attitude of a behaviorist and to speak behaviorese.

Of course introspection is for the behaviorist a form of language behavior. It is, by the way, a curious fact that in spite of the signal importance which language behavior assumes in the behaviorist's system (thinking being subvocal talking), the adherents of this system have so far shown little interest in the experimental work that has been done on language. The literature of memory experimentation is, from their point of view, of course, a literature dealing with the formation of language habits, and as such full of significance, yet Watson's treatment of this literature in his 'Psychology,' for example, is not only far less adequate than his discussion of work in other fields, but in one important instance, his statement regarding the confirmation by later workers of Ebbinghaus's finding that the number of repetitions increases more rapidly than the amount of material to be learned, it is incorrect.¹ Yet nearly all of the experimental

work on memory has been done under scientific conditions and well deserves to be assimilated into the body of facts with which the behaviorist must deal. His break with traditional psychology, however, is so recent that he is happier on the field of salivary reflexes and extensor thrusts than in any region where introspective psychologists are likely to be met.

But this is a digression. The problem of introspection from the behaviorist's point of view is not concerned with the investigation of language habits for their own sake. Introspection to the behaviorist is not simply language behavior, but language behavior regarded as evidence of other behavior. The introspective side of memory investigations, for instance, consists not in the speaking of nonsense syllables or verses of poetry, but in the speaking of remarks about visual or kinaesthetic imagery, distractions of attention, feelings of satisfaction or dissatisfaction accompanying the working of the language habits principally involved in the learning. Professor Weiss,² in an otherwise admirable study of the relations of behaviorism and functional psychology, calls introspection a minor speech reaction. He distinguishes between the major reactions studied in an experiment, and various minor reactions, including vasomotor and respiratory changes, processes in the sense-organ, and speech reactions both actual and incipient, these speech reactions constituting introspection and being to him, apparently, of negligible importance. Such a view overlooks, it seems to me, the fact that speech reaction may be taken as indications of the presence of other types of reaction. The remarks made by a person introspecting would indeed be of minor importance both to the behaviorist and to the introspective psychologist if they indicated nothing but themselves. I should like to

¹ He says on page 337, "Since Ebbinghaus's investigation was completed a large amount of such work has been done, but in general his conclusions have been many times verified." The fact is that all later investigators, Meumann, Weber, Radossawljewitsch, Knors, Reuther and Gamble, far from verifying Ebbinghaus's conclusion, established just the opposite result, namely, that the number of repetitions increases much less rapidly than this amount of material.

² *PSYCHOL. REV.*, 1917, 24, 301-317.

suggest that the behaviorist use as his equivalent to the term introspection the term 'symptomatic language behavior.'

If this expression be accepted, clear thinking will be further helped by an attempt to reach a behavioristic definition of language. Obviously, not all behavior that is symptomatic of other behavior is language. The sounds and movements made by an animal or a human being under the influence of emotion, 'involuntarily,' to use the common term, are not language, because they would be made just the same if no other living being were present. Symptomatic behavior is not language unless a part of the effective stimulus is the presence of some other living being: a cry of pain may have as its sole stimulus the injury that is being inflicted by an inanimate force, but a cry of pain that is also a cry for mercy or help has as a part of its stimulus the behavior of some other living thing, and a cry for mercy or help is language. Further, true language is not dependent wholly on inherited mechanisms, but is determined in part by the effects of previous stimulation of the individual. In some cases even where the stimulus is the behavior of another living being the vocal response is not language: an animal may cry out with pain on being attacked by another animal; such a cry of pain is not language any more than if the stimulus were an inanimate force, because the victim's reaction is purely innate. If on the other hand he cries for mercy or help he is reacting in a way that shows the influence of past situations; in popular terms, to secure results, and not merely because he cannot help it. The following, then, may serve as a behavioristic definition of language: sounds or movements made by the striped muscles of the body, and determined in part by the behavior of other animal organisms and by the effects of past stimulation through the behavior of other animal organisms. The youngster who bawls with one eye on a sympathetic audience has transformed his bawls into true language because his action rests partly on his previous experience of the effect of bawls on the audience's conduct.

It must also be realized that while all symptomatic

reactions are not language, not all language is primarily symptomatic. In ordinary popular parlance, we use language for two purposes. The first is to produce action that satisfies any one of a varied range of our needs, as when we ask persons to perform services for us and give them directions: while such language may be regarded as a symptom of our particular need at the time, its function is not so purely symptomatic as in the second case, where we use language to bring about action *like our own*, as when we describe our experiences to another person. This use of language is based on the special need for sharing experience, for obtaining sympathy; on a peculiarity of socially constituted creatures whereby they are uneasy unless the creatures about them act as they do. True symptomatic language reactions rest on the need for communicating and sharing reactions, quite apart from any ulterior results.

Now, further, is all symptomatic language reaction introspection? The textbooks say that introspection means a looking within. Should it be maintained that symptomatic language reactions are introspective only when the behavior that is shared is inner behavior, referred to the reacting organism itself, and not that behavior which constitutes a description of the stimulus? Shall we say that when a person's language reactions are symptomatic of his inner reactions they are introspective; when on the other hand they are symptomatic of the character of external objects, as when he describes to us a new species of animal, they are not introspective?

Such a distinction between description of process and report of meaning, between true introspection and introspection which commits the 'stimulus error,' is current in structural psychology, but it does not seem to me fundamental for the present discussion. For one thing, as description of an external object becomes more minute and detailed it always tends to pass over from description of object to description of processes set up by the object. We describe an object as hard and hot, the qualities being referred to the object: more detailed attention analyses the hardness into

contact pressure *plus* deep pressure and the heat into warmth *plus* cold or warmth *plus* pain, these being processes set up by the object. Again, whether a given quality (let us forget for the moment that behaviorists do not experience qualities) is regarded as belonging to the stimulus or to the reacting organism depends on whether it is being treated in its setting with other qualities from the external senses, such as colors received from other parts of the object, or in its setting of qualities that are experienced only from within the organism, such as feelings and kinaesthetic sensations. We may, I think, safely maintain that all description is really introspection.

Symptomatic language behavior will then be a fair synonym in behaviorese for introspection. It may be attacked as a scientific method upon two grounds. In the first place, the objection may be made that language is unreliable as an index to other types of behavior; that introspection is untrustworthy. In the second place, it may be said that the types of behavior revealed only by introspection are unimportant and scientifically worthless.

The problem as to the trustworthiness of introspection opens the whole question regarding the limitations of language. Of these the most fundamental is that language, whether of speech or of gesture, is always a system of successive happenings, while the experience it is supposed to communicate involves complex masses of simultaneous happenings. A language cannot be learned unless its symbols are fairly simple, so easily do associative dispositions interfere with and confuse one another; hence the condensed and complicated single moments of reality must in description be drawn out into long series of words and thereby falsified. This defect, however, is far more serious for aesthetics than for science, which deals in any case with a transformed reality. From the point of view of science, the chief requisite is that a given symbol shall always carry the same significance. And the conditions that interfere with the satisfaction of this requirement lie in the circumstances under which language has developed.

These circumstances were, of course, the great practical needs of mankind. Such aspects of the external world and such inner states of the human organism as had most importance for welfare were those to which the reactions of language became attached. Thus the naturally and spontaneously acting human being will use the word 'loud' when acted upon by a certain tone stimulus, and those who hear this vocal response know with very fair approach to certainty what other responses on his part may be expected: 'loud' and 'soft' refer to aspects of the stimulus-response situation that are of practical importance. But a person will not use the expression, 'having the vocality of I,' when acted on by a tone stimulus, unless he has had training in the artificial surroundings of the laboratory, nor will the naïve listener know what he is talking about, that is, what other behavior accompanies the unusual phrase. Similarly, the natural man may say that he is uncertain what to do, and the natural listener infers with reasonable assurance the inner reactions that accompany the words: uncertainty is an inner state of the organism sufficiently important practically to have become firmly associated with its verbal expression. But only the sophisticated frequenter of laboratories will say, 'I experience kinaesthetic imagery,' or, 'I am conscious of a fiat that precedes my action,' and the untrained listener will not know what he would be at, for practical life has created no fixed association between language and such practically unimportant details of the inner attitude of the organism. The more remote from every-day life and its requirements are the reactions which language accompanies, the less trustworthy is language as an index of these reactions, and the greater the danger that different observers will use the same term in different situations and different terms in the same situation.

Further, not only has our vocabulary developed under the limiting influence of practical needs, but our use of the vocabulary we have is modified by a special set of practical conditions, the social instincts: that is, our need of adjusting ourselves in special ways to the behavior of our fellow-beings. Here belong the effects of imitation and suggestion, which

may render a verbal formula symptomatic not of the individual's inner reactions, but merely of the particular external copy that happens to have been set him. Here belong also those modifications of language that spring from the self-exhibiting instinct, the Freudian censor, or, in ordinary language, the desire to appear well in the eyes of others. I will not attempt the hard task of stating in purely behavioristic terms the equivalent of such influences as the self-exhibiting instinct, or the inferiority complex, or the will to power: both behaviorism and structural psychology may well envy functional psychology, or self psychology, their justification in leaving such concepts unanalyzed. But the desire to appear well in other eyes does exist, however it is described, and does produce such mechanisms as displacement, compensation, and symbolism. The working of these mechanisms has been held to invalidate the testimony of introspection in the field of the emotions. I may say at once that there seems to me an error in this view. It is claimed, for instance, that when a person declares that he hates another, psychoanalysis may reveal his true attitude to be one of thwarted love. Now to call the hatred unreal and the thwarted love real involves the assumption that our emotions are not actually determined in part by social conventions and the self-exhibiting instinct. I believe that they really are so determined. When an individual has a certain emotional attitude towards another, this attitude is due in very large part to the whole body of instincts and habits which he brings to bear on the stimulus. The Freudian censor does not repress an emotion: it is a normal part-determinant of the emotion. Suppose that a person hates another because his self-esteem is inhibited by the other's coolness to his previously manifested love; his self-esteem is just as genuinely a part of him as his love: he really does hate. What psychoanalysis does is not to reveal a true emotion and substitute it for a false one: it transforms one emotion into another by abolishing the patient's self-respect and regard for appearances, which are among the normal determinants of emotion. Or to speak more accurately, the psychoanalyst

alters the stimulus for the self-exhibiting instinct in his patient: it was originally the standards of his friends and family, and is now the standards of the psychoanalyst.

The various disturbing influences which affect the trustworthiness of symptomatic language reactions will vary in the amount of harm they do according to the degree in which such language reactions can be correlated with other reactions. There are cases where the behavior of external objects bears out the testimony of symptomatic language reactions. Examples are almost too numerous to mention. Such verbal responses as 'blurred' or 'distinct' made to stimulation by light can be shown by experiments with lenses and screens to correspond with actual peculiarities of the image cast on the retina. When an observer reports that fainter and higher tones accompany the fundamental tone produced by setting a metal plate into vibration, sand strewn on the plate will show that it actually does vibrate in fractional parts as well as in one piece. There are other cases where the symptomatic language reaction can be correlated, not indeed with physical observations of the stimulus, but with other observable reactions of the organism. Not infrequently the direction of attention, for instance, can be inferred from signs other than the observer's verbal report of it. Emotional states of course have a variety of manifestations other than language, although unfortunately most of the easily observable signs of emotion are so readily controllable that when they are allowed to appear they practically constitute gesture language. Sometimes one set of language reports can be compared and checked against a different type of language report: thus Friedländer¹ and Fernberger² have shown that differences in the discrimination of weights (these differences of course being expressed in language) are regularly correlated with differences in the attitude of the observer, introspectively reported. Now where there exists a regular correlation between symptomatic language reactions and other symptoms, the trustworthiness of introspection cannot be seriously impugned.

¹ *Zsch. f. Psychol.*, 1920, 83, 129-210.

² *J. of Exper. Psychol.*, 1921, 4, 63-76.

There remains, however, a considerable number of phenomena where symptomatic language reactions are unconfirmed by any other sort of evidence. Here, certainly the sources of error which affect language will have nearly unchecked play. The behaviorist may well say that in these cases the expression 'symptomatic language reactions' is inappropriate, for these language reactions are symptomatic of nothing but themselves. A doctor calls an increase of body temperature a symptom of infection, but he has other means of verifying the existence of infections besides the behavior of a thermometer.

There is no denying that these criticisms have weight. Let us see, however, if mitigating circumstances can not be found. To begin with, and parenthetically, as it were, we must recognize that in this class of phenomena, whose existence rests wholly and entirely on the testimony of language, there are included a number which are so common, so generally agreed upon, that the burden of proof is thrown justly upon the person who denies their existence. The mental image is of course the most striking instance of this. The testimony of mankind to its reality as an experience, though only verbal testimony, goes far towards balancing the purely theoretical objections of the behaviorists. But there are other examples besides the mental image: we have no evidence but introspection for the existence of after-images, of binocular double images, of flicker effects, of temperature and pressure spots on the skin, of the fusion into one of two touch impressions in the Weber compass experiments, and of a number of other comparatively simple observations. Secondly, it must be noted that rejection of the whole group of phenomena for whose existence symptomatic language is the only evidence would be inevitable if the language used in connection with them were not the same language which is used in connection with reactions resting on other evidence. But of course it is the same; all our descriptive terms are derived from externally observable responses. When a person is engaged in introspecting his organic and kinaesthetic responses, which are not externally observable, he has to use

such expressions as strain, pressure, warmth, contraction, which apply equally to his reactions to external stimuli. If the meaning of these terms is well established in connection with externally observable responses, it may indeed be rash to conclude that the inner responses are exactly like the externally observable ones, but it certainly seems safe to infer that the use of the same term implies a common element. Köhler,¹ by the way, in closing the report of his investigation on auditory sensation, makes an interesting suggestion which bears on this point. He proposes the hypothesis that what he calls the compelling tendency to call high tones 'bright' and low tones 'dark' means a real similarity between the processes in auditory and visual cortical centres. Since the term 'bright' is also applied to touch sensations by some investigators, it would, if there is anything in Köhler's suggestion, indicate a peculiarity of the nervous processes that may be common to three cortical regions, sight, hearing, and touch. Our present interest in this idea of Köhler's is due to the firm reliance it shows on the existence of processes for which no other indication but language can be found.

These considerations, however, can only mitigate, they cannot abolish, the difficulty of finding clear and unambiguous terms to indicate the subtler reactions. The precaution adopted by those who use the introspective method consists in giving the observers a careful preliminary drill in the use of terms. Now this proceeding, of course, provides immediate occasion for the behavioristic enemy to blaspheme: he points to the obvious danger from suggestion. The introspective psychologist may, however, say in defense that other descriptive sciences also have to train their inexperienced observers in the use of terms. Take, for example, the terminology used in reporting the very difficult observations on the structure of protoplasm; or such astronomical observations as those on double stars, needing years of practice; take the observations of planetary markings, or, in chemistry, the use of the Wheatstone bridge, which requires liminal discriminations of noise. These observations are wholly analogous

¹ *Zsch. f. Psychol.*, 1915, 72, 182.

to certain types of introspective work, where an actual external stimulus is given, but where the reactions to that stimulus are complicated and may vary greatly according to the degree in which the stimulus is reacted to in its details. Compare the careful introspective observations of Boring¹ on cutaneous sensations or of Murray² on the qualitative analysis of the sensation of tickle, or of Köhler³ or Rich⁴ on the properties of tone sensations. Whatever sources of error affect such language reactions as these studies involve must be admitted to apply equally to any difficult observations in the other observational sciences.

But there is one type of introspective work that does occupy a position more exposed to falsifying influences than any other scientific investigation. It is the type where there is no external stimulus at all. A trained and an untrained observer may vary greatly in their reports of double stars or of a cutaneous sensation, but at least we do know that the two observers are being acted upon by the same external force: there is one common factor in the situation. When the language report relates to purely internal processes like conscious attitudes, complex feelings, imageless thought, there being no external stimulus, we have no positive guarantee of any common factor in the situation at all. Persistent and spontaneous use of the same language by different observers, as in the case of the mental image, gives presumptive evidence of similarity of inner reaction, but it must be admitted that this is the realm where introspection is most vulnerable.

We may turn from the problem of the trustworthiness of symptomatic language reactions to that of the value of what they tell us.

The proper introduction to a discussion regarding the importance of the information gained through a psychological method is a definition of what constitutes important infor-

¹ *Quarterly Journal of Experimental Physiology*, 1916, 10, 1-95.

² *Amer. J. of Psychol.*, 1908, 19, 289-344.

³ *Zsch. f. Psychol.*, 1910, 54, 241-289; 1911, 58, 59-140; 1913, 64, 92-105; 1915, 72, 1-192.

⁴ *Amer. J. of Psychol.*, 1919, 30, 121-164.

mation. There are, it would appear, three directions in which the results of a psychological experiment may be important. First, they may be useful in practical life; thus an improved method of testing intelligence, or the discovery that a given method of advertising brings in more results than another method, or a demonstration that fatigue sets in sooner when a task is performed in a certain way, is practically important. Secondly, a psychological investigation is important if it throws any light on the workings of the nervous system; on the functioning either of sense organs or of the sensorimotor system as a whole. Thirdly, to psychologists of the non-behavioristic schools, the result of a psychological investigation is important if it conduces to the better description of a mental process. To the structural psychologist especially the analysis and description of mental processes is worth while for its own sake, apart from its relation to practical welfare, and even apart from its immediate results on knowledge of nervous function. The functional psychologist does not share this interest in the mere description of mental processes, although he believes in their existence and in their capital importance for the welfare of the organism. The behaviorist, of course, denies that mental processes exist. He will, however, properly be interested in any language reactions that furnish information bearing on practical needs, or on the functioning of the nervous system. From the behaviorist's point of view, then, the question as to the worth of introspective results reduces to the following terms: Can any information be gleaned from the language reactions which accompany inner and less accessible reactions of the organism, that bears either on the working of the nervous system, or on the practical responses of the individual to the needs of life?

We have noted the fact that there are certain reactions of the organism, as for example those involved in attention and emotion, where the testimony of language may be confirmed or corrected by that of other symptoms; while there are other reactions for which language furnishes the only accessible evidence. In the case of the first type, the

value of symptomatic language reactions both for practice and for physiological theory cannot be disputed. The convenience of language as an indication of the direction of attention in a person undergoing mental tests, as evidence of the nature of an advertisement's appeal, as indicating the early stages of fatigue in experiments to determine the effect of various working conditions on efficiency, is too obvious to need discussion. As for the emotions, Professor Perry says, indeed, that, "almost every recent advance in this field has resulted from the more or less complete abandonment of the introspective method of description"¹; but I do not know how far the Freudian psychology would have progressed if it had had to deal with animals incapable of language: most of its evidence seems to involve symptomatic language reactions. And that bulwark of behaviorism, the James-Lange theory of emotions, makes its most convincing appeals always to introspective evidence.

If we turn to the group of phenomena for which language is the only evidence, we find, as has been seen, that they may be subdivided into those which like after-images, binocular double images, difference tones, have an immediate relation to a known external stimulus, and those which like mental imagery and thought processes have a much remoter relation to outside stimuli. Now two examples, at least, will occur to us where phenomena of the first sub-class have been used to support important conclusions regarding the functioning of the nervous system. The first is Sherrington's appeal to observations on binocular and monocular flicker. There is absolutely no way of determining the existence of flicker effects except by language reports. Sherrington found, it will be recalled, that when the two eyes are separately stimulated by interrupted flashes of white light, in such a way that the flash received by one retina coincides in time with the dark interval on the corresponding point of the other retina, there is no interference or summation effect between the two phases in the two eyes. Hence he draws the conclusion, of the greatest possible significance for the theory of nervous action,

¹ *J. of Phil.*, 1921, 28, page 92.

that while the fact of the connection of the right half of each retina with the left half of the cerebrum means a final common path for the control of movements, it does not mean a final common path so far as sensory processes are concerned. And on the basis of these observations he warns us "against any hasty conclusion that the neural mechanisms which synthesize reflex movements illustrate in their arrangement also those concerned where sensual fusion is the phenomenon."¹ This warning of course applies especially to the behaviorist.

Again, take the blending of two touch impressions into one: the Weber compass experiment. It is evidenced in no other way than by language reactions, it is a typical introspective datum, and of course one of the oldest in experimental psychology. The behaviorist will recall that it has recently played a leading rôle in the construction of an elaborate theory of the working of the nervous system. Head makes great use of it in developing his hypothesis regarding the relative functions of the thalamus and the cortex; namely, that the cortex is the organ of discriminative sensibility, while the thalamus gives sensory impressions that are poorly discriminated. Head² tested patients with variously located brain lesions, and found that the ability to distinguish two compass points was lost when the cortex was thrown out of function. Boring's³ successful attack on Head's earlier observations by the compass method renders the accuracy of Head's observations on pathological cases highly doubtful. But it will be remembered that the issue between Head and Boring on the compass point experiment rested not on a difference in introspective testimony, in which case the enemy of introspection might have an excuse for ignoring the controversy, but on a difference in the mode of applying the compass points. Head,⁴ in his earlier observations on his own sensations during recovery from section of a

¹ 'Integrative Action of the Nervous System,' 1906, 386.

² *Brain*, 1918, 41, 58-253.

³ *Loc. cit.*

⁴ *Brain*, 1908, 31, 323-450.

cutaneous nerve, reported entire loss of the power to discriminate compass points at ordinary separations, and deduced from this fact the existence of a special form of sensibility which he called protopathic and later referred to the thalamus. Boring, after a similar nerve section, found the two-point threshold not affected at all, and explains Head's results as due to lack of adequate control of the pressure exerted by the compass points. What interests us is that here is a purely introspective phenomenon which has assumed capital importance for a theory of the nervous system. It does not matter that anyone who reads Head's articles is likely to think that never was a huger theory built on a shallower foundation of facts: the trouble is not that Head's evidence is introspective, but that he gives us so little assurance regarding the precautions he has observed in obtaining it.

The behaviorist will be most likely to reject as unimportant for his purposes those introspective studies where there is less immediate reference to an external stimulus. Take for example the work of Moore,¹ Weld,² and Tolman³ on the relations between meaning and image, or the group of studies made at Clark under Baird's direction, whose object was to describe the stages in a mental operation and the changes which they undergo as the mental operation becomes more practiced; the work of Woods⁴ on recognition, of Fisher⁵ on generalization, or of Crosland⁶ on forgetting. No doubt from the behaviorist's point of view, the fact that persons should actually pay money for the opportunity as research students to perform and induce others to perform language reactions so remote from contact with any kind of reality, and that they should be given for such performances doctor's degrees which have a money value, though a very slight one, must appear a behavior phenomenon abnormal enough to throw light on many problems of morbid psychology. Need-

¹ *Psychol. Rev.*, 1915, 22, 177-225.

² 'Studies in Psychology in honor of E. B. Titchener,' 1917, 181-208.

³ *Psychol. Rev.*, 1917, 24, 114-138.

⁴ *Amer. J. of Psychol.*, 1915, 26, 313-387.

⁵ *Psychol. Monog.*, 1916 (no. 90).

⁶ *Psychol. Monog.*, 1921 (no. 130).

less to say, this large body of investigation was not performed with the object of interesting the behaviorist, but in the belief that mental processes really exist and therefore are worth analysis and description. But is there really nothing here for the behaviorist except a challenge to find out how people come to be so foolish?

Practical significance for ordinary life this work obviously lacks, although conclusions of a practical bearing might be drawn here and there if the investigators were interested to draw them. For example, Weiss¹ criticises Fisher's work on the process of classifying because it does not help a man to classify any better to know that in classifying "the regions of essential group features were stressed in consciousness and these regions behaved in consciousness in a fashion which depended upon their resemblance or lack of resemblance to the corresponding features in the group members. In the former event, the regions in question passed in and out of consciousness in rapid and ready fashion, without retarding the course of attention. In the latter case, on the other hand, the course of attention was arrested sharply; these regions often persisted in consciousness, and they were frequently accompanied sooner or later by more or less focal and intensive kinaesthetic, organic, and affective contents which functioned in their conscious settings as definite rejections of the figure."² However, Fisher's finding that there are two types of classifiers, one more cautious and the other with a more pronounced 'response tendency' might have yielded practical advice on the best method of classifying, if the author's aim had been practical: she notes that the 'response-tendency' classifiers were the more rapid, and probably could also have furnished data on the comparative accuracy of the two types.

Now it seems to me that although this type of introspective work is directed primarily towards an end of which the behaviorist does not recognize the value, it has nevertheless contributed very largely to a result which is of the utmost worth to the behaviorist's theories as to the working of the nervous

¹ *Psychol. Rev.*, 1917, 24, 364-365.

² *Amer. J. of Psychol.*, 1917, 28, 115.

system. This result is the recognition of the importance of kinaesthetic processes.

The whole behavioristic theory of learning rests, of course, upon kinaesthesia, or, to use a more objective term, upon proprioceptive processes. It reduces all forms of association to habit, and habit means such a linking of nervous pathways that the performance of one movement stimulates, by what Sherrington called proprioceptive paths, the performance of the next movement in the series. What is the evidence that this explanation, which I myself believe the true one, is correct? Watson, who of course holds the theory without question, offers so far as I can see no evidence for it except the anatomical fact that there are sensory endings in muscle. Sherrington's observations indicate the importance of proprioceptive processes in the maintenance of tonus, but Sherrington does not discuss habit formation at all. Do we not really owe largely to introspection our belief that in a thoroughly established habit kinaesthetic processes play the leading rôle? That is, in behavioristic terms, is it not the evidence of language reactions associated with the contraction of the muscles of various parts of the body which is most important for the theory that habit and association rest on a kinaesthetic or proprioceptive basis? I feel quite sure that such language reactions are the main evidence of the existence of what Watson calls 'implicit language habits'; or subvocal talking: it is, the introspectionists would say, because we can feel ourselves subvocally talking under certain circumstances that the doctrine is plausible; or, translated into behaviorese, because we make certain explicit speech reactions which report the existence of these slight and incipient speech reactions. Now it is precisely the highly trained and sophisticated type of introspection that has done most to extend our conception of the rôle played by proprioceptive excitations. To be sure, a hundred years ago, Thomas Brown, using introspection of the old arm-chair kind, devoid of modern subtleties, declared in the strongest terms that "our muscular frame is not merely a part of the living machinery of motion, but is also truly an

organ of sense."¹ But how extensive the effect of this sense-organ is could not be revealed by ordinary introspection, because kinaesthetic excitations do not as a rule demand attention on practical grounds, and symptomatic language reaction, or introspection, as we have seen, has developed like all language under the pressure of practical needs.

I confess that to me, although I believe in the existence of mental processes and therefore think their precise description worth while for its own sake, the most interesting results obtained from the more elaborate pieces of introspective research are those which bring into prominence the rôle of kinaesthesia. It is hardly necessary to give examples of the way in which recent work constantly does this. An investigation to determine whether a certain experience shall be classified as 'attribute' or 'meaning' decides for the latter term because the experience in question is 'carried' by kinaesthetic and organic processes. Descriptions of successive stages in various mental operations contain a high percentage of allusions to the appearance and disappearance of kinaesthesia. If the behaviorist wishes to show that the so-called higher mental processes can be supplanted on his theory by systems of movements, he will have to turn his attention to these introspective studies, or else repeat them himself, for nothing but symptomatic language reactions will furnish evidence of the proprioceptive processes required for such an explanation.

Introspection, or symptomatic language behavior, appears thus to give results as trustworthy as those accepted in other descriptive sciences, which themselves, indeed, often rely on introspective evidence. It is most vulnerable in cases where the reactions reported by language alone have no immediate external stimulus. It has furnished data which the behaviorist cannot afford to disregard; which, in fact, he takes for granted. We learn from the highest of all ethical authorities that the peacemaker is blessed, but experience shows that

¹ Extracted in Rand's 'The Classical Psychologists' from Brown's 'Treatise on the Philosophy of the Human Mind,' 1827, Part II, Chapter V, Section I. The parallel passage in his 'Lectures on the Philosophy of the Human Mind,' 1824, is 1, 461.

the blessing is seldom bestowed upon him by either of the previously contending parties. So this address will seem to the introspective psychologist a pitifully inadequate defense of introspection, while the behaviorist will pay no heed to it, preferring to cut his garments from new cloth rather than thriftily to salvage what of last year's wardrobe can still be worn. Its writer, however, felt the need of clarifying her own mind on the points which have been discussed, and the audience must console itself with the thought that she, at least, has gained some satisfaction from the process.

THE CRUX OF THE PSYCHOLOGICAL PROBLEM

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Psychology began in the field of consciousness as 'mental philosophy.' In general it has gravitated toward the positive methods of science. To at least one school it has gravitated entirely outside consciousness into bodily behavior. Between these two poles of psychology—'mental philosophy' and 'behaviorism'—there is room for many differences of opinion and much discussion.

Amid voluminous discussion in psychology it is perhaps our first need merely to locate the central problem in a more significant manner than is usually done. It is the aim of this brief statement to give the main problem of psychology a clear and definitive shaping and attempt to point out, on the basis of success in practice so far, the direction in which a solution might be sought.

The central problem of psychology is the problem of conscious 'thinking' reflection. That man possesses this power is denied by no one. Until this power is clearly assessed with reference to its function in man's adaptive skill, there is an obvious and disturbing 'unknown quantity' in human psychology which no amount of dogmatism can either evaluate or exorcise. Moreover all human psychology comes under the spell of this 'unknown' element. All that we know about psychology is *conscious* knowledge. It is a body of fact that we 'know that we know' and until we have discovered exactly what 'knowing that we know' contributes to learning and adaptation, a mist hangs low over all the facts. In brief, there is an *X* in human psychology and it is everywhere present in the science.

Nor is this 'unknown' factor pertinent within the problems of human psychology alone, as comparative psychology

abundantly shows. Science is of course as such possessed by *conscious* minds. But sciences, other than psychology, are, it seems, not so seriously embarrassed by the fact that we do not know the function of conscious mind. They may assume consciousness with some impunity. Psychology, however, cannot do this. *Consciousness is its first datum*. Until we know how to interpret this datum with reference to psychology as a whole the science is a good deal at the mercy of the personal equation of the psychologist. According to the temper of the particular psychologist consciousness may be projected into all parts of psychology or ejected entirely as of no scientific moment. Thus we have the doctrine of 'entelechy,' 'cell-soul' and less mystic views of lower-level consciousness standing in juxtaposition with thoroughgoing mechanism which denies to consciousness any function whatever,—and each respective position unassailable except by an attack as doctrinaire as the position itself. This situation promises to continue until *we can draw a clear line marking the relation between organic types of learning and human consciousness*.

In order to pursue our aim to give this central problem a clearer statement we shall first try to evaluate the strictly practical elements in the major controversy between orthodox psychology and behaviorism by separating out those facts of present scientific importance from the large body of controversy which is for the present of theoretic import only. This procedure does not of course assume that the theoretic considerations are unimportant. Our step is assumed to be of adventitious value only,—a mere matter of good pragmatic approach.

It must be patent to anyone who has followed the controversy between behaviorism and the older views that the argument all develops within the technically rather narrow field which in common usage is called 'thinking' or 'reflection.' It is only less clear that although the issue develops at several points in the supposed facts it roots back in the assumptions of the respective positions. In other words the

differences arise in distinctively human psychology—'reflection'—and the answer which each school makes is *appropriate to its assumptions*. It is quite necessary to see this in order to take away the *semblance of objectivity* from a good deal of argument which is in the main strictly theoretic. Let us look at the major assumptions of the two positions.

The older psychology accepts consciousness as a *bona fide* datum, and accordingly reckons with the content it offers, sensation, image, concept, meaning, purpose, etc. It is not however proved that consciousness is scientifically relevant.

The behaviorist on the other hand defines stimuli and response, situation and adjustment, in purely physiological terms, and by this means makes it irrelevant whether or not the subject responds in the field of consciousness. Thought is then identical with its expression—at least for scientific purposes. This makes consciousness as objective to psychology as it is to the other sciences, and rules out its content as irrelevant. This procedure the behaviorist justifies on the grounds that it has already been standardized by other sciences. Other sciences assume an observer and his ability to make an observation.

Now we are merely urging at this point that the significant thing about these respective views is the assumptions. If we were to accept the assumptions of either of these positions in a thoroughgoing manner we would probably not find serious fault with the details of the argument of the position chosen; for each theory gives a fairly rigorous application of its assumptions to the details of the problem. This appears to be quite clear in the fact that neither side has truth of undoubted scientific value which cannot be interpreted according to the alternative position. The real differences then are in the *theoretic approach*, and in order to get at them as such we must raise the previous question from the argument back to the assumptions.

When we get back to premises the theoretic advantage of the older psychology is too manifest to need defense or profit by it. Behaviorism in distinctively human psychology is highly doctrinaire. It appears to have no better defense for

ruling out consciousness than a superficial appeal to an associative comparison with the practice of other sciences. This will not do. By no stretch of theory could we hold that consciousness is a datum of any other science than psychology, but it is entirely natural to consider consciousness the *first* datum of psychology whether or not it finally proves to be of scientific relevancy.

But even if we pass by this serious weakness in the theoretic armor of behaviorism in the realm of 'thinking,' we have its own admissions that within this realm 'behaviorism' is *no more than a doctrine*. Watson grants that we do not possess knowledge of the relation existing between thinking and concomitant bodily activity, and admits that "the time seems far off when such a thing is possible." When we recall that the whole attack against behaviorism is made at this point, it seems clear that we should call for an armistice on the ground that there can be no worthwhile scientific issue where it is impossible to make a stronger case against the opponent than he makes against himself. If we are prepared to grant this conclusion it will release our attention to strictly practical considerations of the controversy—considerations which have received too little attention because of the preoccupation with theoretic matters.

From a practical point of view the situation is quite different. Such advantage as behaviorism has is practical advantage. The wings which its opponents have been trying to clip never got it any progress. It has been walking on its legs. Its manifest theoretic handicap has not proved serious from a practical point of view at least for the present. It is difficult to make a scientific case against a theory in a part of the field in which no theory is making progress. If behaviorism seems logically inadequate for distinctively human psychology, the older psychology has at least failed to be adequate in practice. Meanwhile in the field of animal psychology and that large part of human psychology which appears to have the same pattern as animal psychology, the methods of behaviorism have revealed a distinct advantage over older methods. We may shake our heads when told

that behaviorism, as at present defined, is the only scientific way in psychology. We cannot deny however that it has made progress where the older methods were lost in confusion. Nor can we claim that this method is reaching a margin of diminishing returns—a claim which might be made with much plausibility of the older methods provided we mean really scientific returns. Behaviorism is making progress where it is reaching its accepted data and that which it neglects in human psychology is not proved to be significant from a scientific point of view.

Briefly then the practical situation appears to be this. Neither the introspectionist nor the behaviorist is making any sure progress in distinctively human psychology—that is, human psychology as contrasted with animal psychology.¹ This is the very point at which they are at issue with each other. This issue is then for the present purely theoretic. As such the older psychology has the advantage. But this logical advantage is not yet a practical one; for the behaviorist's 'end result' view offers a type of explanation for all facts of proved value in the disputed field. Meanwhile his theory has the advantage in practice elsewhere in psychology. All this lends some weight to the bold assertion of the behaviorist to the effect that the supposed problems which this theory is neglecting will all "be sucked under never to return again" by the normal advance of science.

The writer sympathizes with the theoretic objections urged against behaviorism as applied to human psychology. We must not however let these objections blind us to practical considerations; especially is this true since it appears clear that *within the field of the psychology of thinking, known facts—or at least agreed-upon facts—are too few to make an attack at this point by the older methods lead to determinate results.* On this account the case against the behaviorist at best never

¹Watson subdivides 'thinking' as follows: (1) The mere unwinding of vocal habits. (2) The solving of problems not new. (3) The solving of new problems. Number three represents the area in which behaviorism is attacked when the attack is discriminating. Whatever definite elements are found in one and two, they fall within the general laws of learning found at lower levels also.

gets beyond the stage of probability, and in no case does the accumulated argument become so impressive against behaviorism as does the *mere setting of respective assumptions in juxtaposition*. We shall try to illustrate the truth of this statement further by brief reference to a symposium on the psychology of thinking in the October, 1920, number of *The British Journal of Psychology*. The findings which this symposium reveals will perhaps enforce the opinion that we are shut up by the facts of the situation in this controversy to a more pragmatic attack upon the problem than that which we have been making.

The subject of the symposium is "Is Thinking Merely the Action of Language Mechanism." Brief articles are contributed by T. C. Bartlett and E. M. Smith, Godfrey H. Thomson, T. H. Pear, Arthur Robinson, and a rejoinder by John B. Watson whose views, as expressed in Chapter 9 of his book "Psychology from the Standpoint of a Behaviorist," are especially combated by the other writers.

We shall not attempt to do justice to the suggestiveness of the various articles nor the full position of the writers. It will be sufficient for our purposes to set down the three most fundamental questions which arise in the controversy between the behaviorists and their opponents and note how far the different writers—especially those who are colleagues—agree among themselves as to the facts. The following questions appear to be primary.

1. How far does 'substitution' account for thinking?
2. What is thinking?
3. What is the relation of thinking as response to its expression?

Viewing the symposium as a whole these questions stand out as fundamental to the controversy. Let us take the questions up in order and note briefly the position of each writer upon each question. For the sake of brevity we shall refer to the questions by number and the authors by the initials of their surnames.

1. B. and S. find the behaviorists' notion of 'substitution' inadequate. We need 'substitute-sign' to satisfy the facts.

The use of the latter is not however peculiar to thinking. T. objects to the criticisms of substitution offered by B. and S. and defends substitution as adequate. P. expresses no view on this point. R. also finds substitution inadequate and holds that nothing beyond this is possible under the assumptions of behaviorism. W. as a behaviorist defends substitution as an explanation of thinking. Upon question number one, which has to do with the mechanical technic of thinking, two papers favor substitution, two deem it inadequate, and one offers no opinion.

2. B. and S. hold that the unique characteristic in the thinking response is that it remains constant in many settings. Thinking is a response to abstract qualities. T. objects to defining thinking as response to abstract qualities. This is not peculiar to thinking. Animals also respond to abstract qualities. Thinking is a search for the relevant point in a situation and is in this sense response to abstract qualities. False starts are the very essence of thinking. It is trial and error by proxy in which substitute signs are used instead of actual bodily trials. P. believes thinking is a process beyond definition perhaps. It is to be contrasted with the mere rehearsal of experience and with habits. R. expresses no opinion. W. says, "The whole man thinks in his whole body in each and every part." This bodily activity is at a maximum in the laryngeal region and minimum in the larger musculature. Thinking is never a response to abstract qualities, but always a response to "definite and particular things." Thinking may be divided into, "(1) the mere rehearsal of vocal habits, (2) solving problems not new, (3) solving new problems." Number 3 has alone become identified with 'thinking,' but without defense. A man's behavior in solving a new problem is just like that of the rat in the maze.

Upon this question, number 2, there is no clear agreement. There is at best, approach to agreement in minor matters. The behaviorist, in accordance with his assumptions, makes thinking more inclusive than the others. While the latter all limit thinking to that activity which Watson calls "solving new problems," they differ among themselves as to

what are the significant points of the process and even contradict each other in this matter.

3. B. and S. believe the behaviorist not justified in identifying thought and its expression. They grant however that *thought can be studied in only a very meager way apart from its expression*. It is perhaps always a response to qualities and relations as such. This Watson denies and apparently Thomson also. T. implies that thought is not to be identified with its expression. P. says that thought and its expression are to be sharply contrasted, as much so as skating and figure skating. R. expresses no definite opinion but his whole position is against the identification of thought and its expression, it seems. W. declares that only the expression is significant.

Upon this question all agree in opposing the behaviorists' position on theoretic grounds. B. and S. venture to look at the practical side of the problem and in doing so all but agree with Watson. The other papers do not ask the practical question *as to how we are to get at that part of thought which is not in its expression*.

Of course matters outside of the scope of these three questions are touched upon in the symposium. These questions however cover the points most common to the papers and express fairly well the major issues of the problem. It should be noted that with respect to the questions there is almost no clear agreement of a definitive sort. Such agreement as appears lacks pointedness. Moreover question 2, which is central and put in positive form, reveals no notable agreement even among colleagues. This absence of agreement on the fundamental question as to the significant characteristics of thinking tends to show that the positions taken are on the whole *projected by the psychologist* rather than *objectified from the facts*. If the latter, we would surely expect repetitions (agreements). There seems to be something quite arbitrary about the point of approach chosen by the writers and quite arbitrary in what is held up as significant.¹

¹ Thomson appears to the writer to have come far closest to the correct view of thinking in his notion of "search for the relevant point in a situation" and his "trial

The overweening influence of the theoretic appears also in the answers given to question three. All four papers oppose Watson's identification of 'thinking' with its 'expression.' Only Bartlett and Smith however face the practical problem which this opposition brings to the fore,—that is, "How are we to get at in an objective manner that 'thinking' which is not in its 'expression'?" Bartlett and Smith ask how far we can study thought "apart from its expression," and conclude that it is possible only in a very meager way. They suggest that thought is perhaps always a response to abstract qualities and relations as such. This is in part denied by Thomson and categorically as a whole by Watson. It seems to me that Watson is correct.

Now this question raised by Bartlett and Smith is in some form crucial and should have been raised by each writer who denies the adequacy of identifying thought with its expression. The behaviorist surely does not deny that hypothetical objections can be raised against his doctrine of 'end results.' He however answers all such objections by neglect on the grounds that the concept 'expression' exhausts all other concepts for scientific purposes. An attack upon the 'end-result' claims of the behaviorist must therefore show that it is possible to get at that which the behaviorist is declared to be neglecting. No other type of attack really meets his position. If the behaviorist in fact accepts all that can be studied in a scientific manner he accepts all that is significant for science and his position is sound.

It develops then that the behaviorists' position is very weak theoretically in that it arbitrarily rules out consciousness, but that *apart from behaviorism we lack a clear view of any sort as to what thinking really is.* One course is open to us. Since the behaviorist is more successful in practice in psychology outside the phenomena of human thinking, we may leave theoretic considerations out of account for the present and give this practical fact right of way. We propose to do this. We shall get our orientation to the problem from and error by proxy." He does not however offer more than a statement. He neither shows that such 'thinking' is accomplished nor suggests any technic whereby it *theoretically might be accomplished.*

the practical advantage of behaviorism rather than from theoretic considerations. Let us accordingly accept the behaviorists' position first, and then from *within it* ask at what point in this system we would need to provide for distinctively human psychology in case this part of psychology is not adequately provided for in behaviorism as at present defined.

The background of this line of attack is somewhat as follows. Psychology is a unit. The controversy over behaviorism does not develop over its whole area but only within the field of 'thinking' as understood by the older psychology—or what the behaviorist calls "solving new problems" of thought. Since this problem of 'thinking' has at best only an indefinite statement by the school which appears to have the better theoretic position with reference to it, we are shut up to the step of blocking out this problem within the general field of psychology according to the theory which is more successful in practice elsewhere in psychology. By this means we may hope to discover at just what point this theory of behaviorism would need to adjust itself if the older theory be correct. In other words, we might be able to locate the gap in the line of behaviorist psychology which would be supplied by the materials which the older psychology claims behaviorism is leaving out, provided the older school is really correct. Merely locating the problem in this clear way should prove an advantage. Let us attempt it.

Behaviorism appears to limit psychology to the finding of the 'conditions' of responses and their 'adjustments.' This is a view of psychology which all will accept without reservation for the field of animal psychology perhaps. Likewise doubtless all will grant that human psychology is limited to the determination of conditions and their adjustments, provided we do not restrict the word 'adjustment' so as to identify it with 'expression.' This reservation roughly locates the problem. Our question is at this point, "Is there a significant factor between 'conditions' and 'expression' which the behaviorist leaves out of account?" In brief "Is there in unique human psychology an *X* between the behaviorist's

'situation' and his 'expression' which, left out of account, makes his thoroughgoing 'end-result' doctrine a *non sequitur* at this point?"

This question appears to locate the problem. If there is nothing of scientific significance between 'situation' and 'expression,' then the behaviorist is correct. His method, successful elsewhere in psychology, needs but to keep on its normal course, approaching nearer and nearer to its goal of completely objectifying the central problem of psychology also. If there be an *X* here, then behaviorism is inadequate at this point, and this *X* stands for a technic which when assessed will harmonize the two views by correcting the theory of behaviorism at this point and by giving to the older psychology the objectivity needed for scientific clearness.

This shaping of the problem forces the question which is asked by Bartlett and Smith—the practical question as to how we are to approach this possible *X* behind expression. Bartlett and Smith ask how far we can study thought apart from its expression, and decide that it is possible in only a meager way. They suggest rather timidly that perhaps thought is always a response to abstract qualities and relations as such. As we have said, this is denied. It is very difficult to see how thought could be studied apart from its expression, and yet on the other hand if there be an *X* there must be some way to get at it if it be significant. The question raised is perhaps in unhappy form but the inquiry is central in importance.

The situation in which this question places us requires that we challenge the term 'expression' as used. We should not ask, as do Bartlett and Smith, "How far thought can be studied *apart* from its expression,"—a really impossible thing it seems to me,—but, "Can we make a distinction in the term expression which the behaviorist does not make, which will show the way to a more discriminating study of thought *through its expression*?" So long as we limit the meaning of 'expression' to muscular and glandular reaction no such distinction can be made, it seems. For it is quite clear that the *motor* behavior of a man in solving a new problem is on the general pattern of that of the rat in the maze. If we are to

limit expression to muscular activity the behaviorists have won their case, it appears.

But why limit the term in this way? Is it not an abstract and arbitrary limitation—a bit of ‘rationalizing’ which does not fit the facts? Before we turn with this query to the matter of ‘thinking’ and ‘language habits’ in which we are especially interested, it might be well to raise it with reference to confessedly motor behavior—the rat in the maze.

Are the reactions of muscles and glands as such really the only or even the most significant facts in studying the rat in the maze? I think not. The *integrated* behavior of the rat is the most significant. By integrated behavior we mean the integrated reactions which come out of earlier broken and random trials, and which become an ever better adaptation to the motor problem which is being worked out. This is the behavior toward which experience moves through learning, and no amount of purely random muscular reaction would be important to science were it not for the fact that the ‘set’ of the organism with respect to its environmental problem makes for adaptation by *eliminating* some reactions and *integrating* others. This process of abbreviation and integration is not conceded to be wholly muscular and glandular.

Nor is the final learned reaction the result of muscular reaction alone, even if we pass by the problem raised above concerning the nature of the processes of elimination and integration. The adaptive reaction is shaped to the problem of the environment. It comes to be what it is, not alone by muscular reaction but because of structural elements in both the animal itself and its environment. In other words the reactions of the animal in so far as they have a pattern at all are quite as much conditioned by the articulated bony structures of the animal which are not active but responsive, as by its muscles, and the reaction as a whole comes to be as a response to the structure of the particular environment. Reactions do not of themselves have an interest for science. Before they can have such an interest they must show repetitions (patterns). The reactions therefore in which science is interested are both acts and *results*. To treat reactions as

muscular and glandular without further reference is to neglect problems, overlook facts, and after all to seize upon that which in itself does not appear to be significant for science. It seems fair to say that abstracting reactions from the structural facts which provoke them and determine their shaping is an arbitrary and false method.

It will simplify our further discussion if we leave out of account the effects upon reaction of inherited structure and deal only with learning. As we have intimated, the problem of the environment determines the shaping of a learned reaction. It is shaped *from the outside in*. This takes place in two ways, it seems. The *spacial distances* in the maze from 'position of starting' to 'place of turning,' or from 'one turning' to 'another,' stamp their effects into kinesthetic sensation under which the learned reaction is run off. Also points of stimulation in the environment—colored spots, lights, bells, doors, latches, etc.—become outstanding stimuli 'seen' or 'heard,' under which certain problem reactions are run off. Doubtless both processes go on together in motor learning and are more or less present in all cases of motor learning. In some cases as in that of the rat the kinesthetic sensation appears to be more important to learning than stimuli 'seen' or 'heard.' With lower animals generally 'location of stimulus' and 'distance covered' are important in learning. These depend on kinesthetic sensations which in turn have come from the structure of the environment. Likewise reactions that depend on stimuli seen or heard depend on the structure of the environment, and these reactions are even more interesting for present purposes of illustration. Stimuli seen or heard—such as a light or a bell—come to have a more or less *discursive* use as stimuli guiding motor behavior. In this respect they become 'signs' of adaptive behavior,—that is, they come to be an '*organic*' sign system¹ under which certain reactions are run off. This type of behavior reveals the *organic background* of the 'sign system' as we know it in human psychology.

¹ Since kinesthetic learning is based upon constant or repeated facts in the environment quite as much as is learning from stimuli seen or heard, we shall consider only the latter, as it makes more clear the comparison we are making.

Finally, that which most truly expresses an organism is the reaction which *adapts* it as all behavior shows. Such adaptive acts do not come to be merely by muscular reaction, but by acquiring reactions shaped *quantitatively to the environment or guided by an 'organic' sign system learned from stimuli seen or heard in the environment*. Even if it were possible to account for these acquisitions under the head of muscular reaction, we must still face the fact that the particular reaction comes to be what it is by reason of the structure of the environment and is run off under the stimulation of the environment. Since random reactions of muscles are not in themselves important to science but adaptive reactions are both that toward which learning tends and with which science can deal, and since adaptive reactions are both shaped by the environment and run off under environmental stimulation, our notion of 'expression' ought surely to be enlarged to include *reactions with reference to the environment*, and not remain limited to the abstraction 'muscular and glandular reaction.'

It appears that 'expression' should not only include *adaptive* reactions as such but should *especially* mean these as we have implied. This does not seem to be an arbitrary position. All experience of the animal tends toward such reactions. They are a sort of summing up of activities. They possess the elements of repetition and pattern which science can assess. If we get our orientation to the idea of expression from these facts we will not be content to limit it to muscular and glandular reaction. To do so would not only compel us to assume that all the powers by which we receive and transmit stimulation and by which the responses to these are integrated, abbreviated, or eliminated are exhausted by muscular and glandular reaction, but it would compel us to overlook that an adaptive reaction is run off with reference to a problem of the environment and is an *expression to something*.

Therefore to abstract, in a thoroughgoing way, 'expression' from the organic sign system in which it arises and without which it would not come to be what it is, is false to the facts. The adaptive reactions of the rat in the maze are run off point by point in accord with a process of integration and abbrevia-

tion under a constant series of stimuli in the environment. The muscular and glandular reactions are not the most significant thing, for they might be at a maximum in fruitless, random reactions in which no results for science are available. Fitting into the environment is the important thing for the animal and for scientific observation. I suspect that this is not done by reactions which are *wholly* muscular and glandular in the usual sense of the terms, and it is in any case done in a pattern which is conditioned by the static structural parts of the animal and in definite functional reference to high points of the environmental problem. The *adaptive reaction* is the whole, which is vastly more than the sum of its parts.

This less 'conceptual,' more 'organic' view of expression gives to our problem a new face when we come to apply it to the problem of the relation of thinking to language habits, or better still,—consider the relation of 'thinking' to 'expression.' We do not feel so strongly the tyranny of the muscular reaction theory. Language habits do not consist merely of laryngeal reactions with a minimum of larger musculature reaction. The babble of a child comes thenearest to this. Language considered solely as motor reaction is first of all a *functional* reaction which is the result of environmental stimulation, transmission, integration, abbreviation and is used with reference to environmental situations. For this reason alone to abstract it into 'muscular activity' is not the most meaningful method even if true.

But language is a *result* as well as a reaction. It has been conditioned by the vocal chords in its development. So also the bony structure of the head and chest give resonance and furnish the signs of emotional placement. Likewise the language habits have grown up with reference to the ears of the subject and social associates and have been shaped by a great variety of conditions in the organic and inorganic environment. In other words, the most important thing about language behavior is not the muscular and glandular reactions upon which it surely depends, but rather those integrated *sign reactions* which make it what it is—a most valuable tool of adaptation.

Language has become this valuable tool of adaptation surely not because there is any *muscular tendency* toward laryngeal reaction rather than reaction of larger musculature, but because it has been shaped by a constant process of correction to the problems of the environment. It has grown out of the babble of mere vocal noise-making, not because laryngeal reactions tended to become substituted for the more overt muscular reactions but because *language came to be heard* as a sign system most valuable for adaptation. We should therefore in a scientific study of language habits in relation to thinking, shift the emphasis from abstract subtleties of muscular relation apart from function, to *word sign system heard* in which language grew up and by which it functions. If we do this it will be natural to think of language habits as the sign-system behavior of the 'thinking' behavior,—or the *sign system of thinking*.

In brief, let us say then: When we observe any behavior—for example, a rat in a maze—we do not observe it abstracted from the environment and we should not therefore say that 'muscular reaction' exhausts its expression. We observe behavior with reference to the organic sign system which regulates it, and this sign system is built up by reactions which are probably not *all* muscular and it has reference in function *to facts or points in the environment*. The random and meaningless reactions of muscles and glands become fixed into meaningful pattern because of these constant points of stimulation from the environment.

In the same way we should observe thinking not as merely the reaction of the muscular language mechanism but with reference to language *as an audible sign system*. We observe motor behavior with special reference to a sign system *seen*; we observe mental behavior—thinking—with special reference to a sign system *heard*, for this is the manner *in which these respective systems have been built up in function into integrated sign systems*. We merely in the one case assume the *eyes* of the psychologist and in the other his *ears*.¹

To treat expression then from the point of view of its sign

¹ Of course speech is translated into a written sign system and can then be seen.

system is to seize upon the most important approach as the development of behavior indicates. To take this approach is also to take hold upon the most objective data from which to assess behavior. This is equally true in motor and in mental behavior. It is perfectly apparent that language habits do not grow up because there is any muscular tendency for laryngeal reactions to become substituted for larger muscular reactions. The substitution takes place because the sign system of *language heard* has more adaptive value than *motor reaction seen*.

This brings us to the final significant question: Why speak of behaviorism as if it had to do only with muscular and glandular reaction, since in practice the study of behavior always depends for progress not upon muscular reactions as such but upon the other factors we have mentioned, which make these reactions show repetitions (patterns) and thereby make them of scientific importance? This means that behaviorism in practice keeps in mind not muscles and glands alone but the environment and the powers of the organism to synthesize in learning the effects of the stimulations from the high points in the environment. These latter powers involve the use of muscles no doubt, but they appear to involve more. They are in any case enough different from muscular reaction as generally understood to make us hesitate out of both respect for truth and scientific clearness to include them within the general notion of 'muscular reactions.'

But even so, muscular and glandular reaction without reference to constant elements in the structure of the organism and the environment remain random and meaningless. Since all orderly reaction that is not inherited has its pattern marked for it by a sign system from the environment, and all pattern reaction, inherited and learned, has relation to the bony structure of the organism, it is difficult to see how behavior can be correctly described by abstracting it under the notion 'muscular and glandular reaction.'¹

It appears then that 'behaviorism' is not at fault in

¹ We of course do not deny the importance of the elemental approach. The organismal is however more significant.

method but in a too abstract and intellectual definition of data. It is doctrinaire. Not even in lower animals are we limited in actual study of behavior to the reaction of muscles and glands, while, with respect to the human behavior we call thinking, muscular reaction is a fiction *as data* by the admission of the behaviorist himself. The sign system structure of the reaction is both more important to the organism's expression and more accessible to science than any amount of muscular reaction. This means that the *word signs heard* in language are both more important to language function and scientific observation than the muscular reactions involved. We should therefore especially study 'thinking' (I like the word 'reflection' better) as it is objectified in the use of language *as heard*. The use of spoken language *as heard* is as much a matter of behavior and 'expression' as is the pattern reaction of the rat in the maze *as seen*. We must therefore frankly accept as expression *language as heard* and attempt to study thinking *through its expression in its audible sign system—language*.

Just how far this will get us cannot be considered at this time.¹ We at least have the statement of a real problem in the sense that we have both an *observable datum* and an *objective method*. This it seems to me has hitherto not been the case. The data of muscular behavior when we come to distinctively human psychology—the psychology of thinking—is hidden in the depths of physiology beyond the reach of observation. The older psychology on the other hand has depended upon a method of pseudo-subtlety which cannot without radical revision become an objective method. Any other method than that of behaviorism can, it seems to me, have only the semblance of objectivity at best. Behaviorism is *the* scientific method for psychology. It however must be applied consistently to its full data.

The task therefore which we have before us is to discover the technic of 'thinking' especially from the manner in which it is objectified in the use of language as a vocal sign system.

¹ I am working on this problem. The background study 'The Structure of Animal Learning,' appeared in the May number of the *PSYCHOLOGICAL REVIEW*, 1921. An attack upon distinctively human psychology is in process.

This involves eliminating all the effects which appear in language shaping except those which come from the inner technic or structure of thinking; or in other words, it involves isolating the fundamental pattern of language use from those elements which come into the language habits from timely factors or lower types of learning. Success in this task will give us that part of the pattern of language use and habit which comes from the factor of thinking alone—that is, give us the structure of thinking—its technic. It will in short evaluate the *X* which in human thinking lies between 'situation' and 'end result' adjustment.

This task may seem an heroic one because of the subtlety of the factors and the discursive nature of thought and language—its sign system. No one can deny however that it is a definite task. Indeed if we approach the problem involved in a thoroughly pragmatic manner the facts point unmistakably to this shaping of the problem, it seems. The only way to get at the *unique* technic of human psychology in a scientific manner is to observe, with full respect for all the facts, *distinctively* human behavior. Distinctively human behavior is the *adaptive use of a vocal sign system known as language*. We should study 'thinking'—reflection—through this expression. This applies the scientific *method* of behaviorism while it avoids the theoretic weakness of the *doctrine* of behaviorism as we have come to know it.

If from this point of view the problem seems difficult, this is nothing new. If the woods ahead are as dark as ever and appear to be as trailless, our feet are at least upon the ground. We have some place to stand, from which to make a beginning, which it appears to me is not the case with respect to this central problem of psychology, in either the doctrine of behaviorism as it has been defined, or that of the orthodox school.

A PHYSIOLOGICAL-GENETIC THEORY OF FEELING AND EMOTION¹

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Since the important work of Dr. Cannon, published in 1915, there has been little progress in the study of emotion. The James-Lange theory, which has retained the merited esteem of psychologists, has waited long for confirmation at the hands of physiologists. Yet the latter have been inclined to interpret their discoveries as hostile rather than favorable to it. Perhaps the main defect of the James-Lange statement is one of omission. It fails to differentiate the patterns of visceral or somatic response whose return afferents might give a basis for the different emotions felt. It is the purpose of this paper to propose a theory for differentiating the emotions physiologically, and to clarify if possible the obscure basis of pleasure and unpleasantness.

Although introspection upon emotional states is difficult, there is probably a general agreement that all emotions subjectively comprise (1) an affective element, that is they are either pleasant or unpleasant, and (2) a differentiating factor (x) which serves to distinguish between emotions which are alike in respect to the affective component. On the pleasant side the x factor might distinguish joy from love, or conjugal from consanguineal love. On the unpleasant side it would differentiate such states as anger, fear, and grief. We shall take the stand that both these components (the affective and the differential) are made up, according to the James-Lange principle, of bodily sensations resulting from diffuse patterns of response to the stimulating object. The response patterns for the affective and the x components will now be considered in turn.

¹ Read before the American Psychological Association, December 28, 1921.

I. If we search the body for a neuromuscular mechanism suitably correlated with the antagonistic poles of pleasant and unpleasant, we shall find such a mechanism in the autonomic nervous system and the viscera which it supplies. The physiological antagonism lies between the two divisions of the autonomic, namely, the sympathetic (sometimes called the thoracolumbar) and the craniosacral. As is well known from the work of Cannon and earlier investigators, the sympathetic motor effects include checking of digestion and sex activity, accelerating the heart beat, constricting the blood vessels, and reinforcing its own activity by the liberation of autacoids. All of these changes are in opposition to those produced by the craniosacral efferent fibers. Now all the emotions studied by Dr. Cannon and reported as involving sympathetic impulses are clearly *unpleasant* in their affective value. This fact did not receive the emphasis it deserved in interpreting these brilliant investigations. Dr. Cannon himself laid stress upon the apparent identity of the bodily changes in all the major emotions as an argument against the James-Lange theory.¹ We, however, may justly emphasize this very identity as a *support* of the theory, since it provides a common mechanism for precisely that element which anger, fear, and pain *do have in common*, namely *unpleasantness*. The question of a physiological basis for the differentiating factor is, of course, still left open. We return to it presently.

Since the sympathetic division appears thus intimately concerned in unpleasantness, we may regard its antagonist, the craniosacral, as the foundation of the pleasantly toned pattern responses. The craniosacral division innervates those responses which follow immediately upon the attainment of objects of food and sex. Digestion and sex and breeding activities depend upon its unimpeded efferent impulses. The resident sensations arising from the performance of these activities constitute the two primordial pleasures of mankind. Both the craniosacral and the sympathetic divisions are subject to a vast amount of conditioning, so that with in-

¹ Cannon, W. B., 'Bodily Changes in Pain, Hunger, Fear, and Rage,' 275-283.

creasing age the number of affectively toned objects and situations (that is, those evoking affective responses) is enormously increased.

There are, of course, other pleasures beside those of food and sex. We might mention bodily exercise and habit, and also the excitement of games (without anger or worry), as sources of pleasure not clearly involving the craniosacral division. We may be obliged therefore to include in our pleasure-giving mechanism the free-functioning of the somatic effectors through the innervation of the cerebrospinal system itself. It should be noted also that the craniosacral is more closely related to the cerebrospinal system than is the sympathetic. Its fibers do not enter the ganglion chain system, but comprise really cerebral and spinal nerves whose fibers turn inward and supply smooth muscle.

The first part of our theory then states that emotions are fundamentally distinguishable as pleasant and unpleasant, and that these two contrasted qualities are related to two mutually antagonistic processes whose effector movements in the viscera set up characteristic organic sensations which we call affective quality. The craniosacral division of the autonomic, supplemented under certain conditions by the cerebrospinal system, innervates those responses whose return afferent impulses give rise to *pleasantness*. The sympathetic division produces visceral responses which are represented in consciousness as *unpleasantness*.

Aside from experimental knowledge of the autonomic functions, this portion of the theory receives further support from introspective observation.

First, unpleasantness is both more readily identifiable by introspection and more imperative than pleasantness. The unpleasant emotions are also more numerous and more characteristically emotional than the pleasant. This is readily explained by the fact that the sympathetic motor impulses (owing to the resistance of the ganglion bodies) are necessarily stronger than those of the craniosacral division whose action they inhibit. They are also more widely diffused through the viscera, and they support somatic motor activities of a violent and characteristic sort.

Secondly, if unpleasantness results from the effects of sympathetic motor impulses, we should expect that it would be slower of arousal than pleasantness. The delay in the ganglionic synapses, the long stretch of unmyelinated post-ganglionic fibers, and the diffuse character of the sympathetic innervation (as contrasted with the opposite conditions of the craniosacral) would require a considerably longer transmission time for the sympathetic impulses. Our common experience attests the longer latency of unpleasant feelings as compared with the immediate thrill of pleasure derived, for example, from gustatory or erotic stimuli. The case of stumbling in going down stairs is a good example. In the writer's experience there is a sudden reflex recovery of balance; and then, when several steps have been descended, there wells up gradually a mass of unpleasant organic sensations. Annoyance and anger also have a long latent period. A characteristic non-emotive 'fore-period' has been found in extensive collections of introspection upon anger.¹

Thirdly, recent careful investigation indicates that pleasantness and unpleasantness never occur simultaneously, that is, that true 'mixed feelings' do not exist.² This fact is in harmony with the sharp antagonism of autonomic functions which we have assumed to be the basis of these feelings. The more complex states also, such as fear and rage, are in strict antagonism with the love emotions. The sex drive, reciprocally, is one of the strongest agencies in the dispelling of anger in family quarrels. To sum up, we find our first proposition supported on the introspective side by definiteness, imperativeness, localization,³ latent period, and unmixed quality of the affective state.

II. We may return now to the α factor which serves to differentiate the various emotions within a single affective

¹ Richardson, R. F., 'The Psychology and Pedagogy of Anger,' Educ. Monographs, no. 19.

² Young, P. T., 'An Exper. Study of Mixed Feelings,' *Amer. J. of Psychol.*, 1918, 19, 237-271.

³ Young, P. T., 'The Localization of Feeling,' *Amer. J. of Psychol.*, 1918, 19, 420-430. Though Dr. Young seems to consider the feeling quality unlocalizable, the reports of his subjects indicate a considerable consciousness of visceral reaction in U., particularly in the region of the stomach.

class. Accepting Dr. Cannon's dictum of homogeneity of the visceral changes, we propose that the sensations which comprise the differentiating factor arise from stimulation of the proprioceptors in the muscles, tendons, and joints of the somatic part of the organism. Kinaesthetic impulses resulting from the emotional response deserve as true a place as organic impulses in the conscious fusion. Different, and in many respects antagonistic, somatic effectors are brought into play according to whether the individual attacks or flees. Facial expressions as well as bodily movements are strongly differential. The view proposed is that afferent impulses from these somatic response patterns add the characteristic complex of sensations through which an emotion of fear is different in its consciousness from an emotion of anger. The cerebrospinal or projicient activity of adjustment to the situation thus supplements in consciousness the sympathetic core of pure feeling.

Let us appeal again to familiar experiences for elucidation of our second proposition. Attitudes are generally taken, or overt responses quickly made in nearly all emotional situations. In repression, where all somatic response is inhibited, the emotion is simplified and reduced to its foundation of visceral unpleasantness. The unpleasantness is probably increased by the very blocking of the somatic outlets. We may consider also the temporal relations of the two components in emotion. In the case of stumbling on the stair the starting response was completed before the sympathetic affective component was felt. The emotion therefore was not true fear, but simply an intense unpleasantness. To take another example, when the objective situation causing anger or embarrassment has been removed, the visceral component, being more sluggish than the somatic, outlasts the latter in the form of a purely unpleasant affective state which delays the full recovery of composure. If a wild animal is held in the hand, or if a child is pursued and brought to bay, there will follow a quick alternation or succession of attempts at escape (whose emotion is fear) and desperate attack (with the emotion of anger). The shift from intense

fear to intense rage is too sudden to admit of a complete change in the visceral pattern. We may quite plausibly attribute it to the quick change in the response pattern of the striped muscle, superimposed upon the constant visceral undercurrent of unpleasant affectivity.

If some reminiscence of James's arguments be permitted, we may recall the impossibility of separating introspectively the somatic factors, the frown, clenched fist, etc., from the emotional experience. When we abstract from these only affectivity remains. As to the pleasant emotions, we may assert that the differentiating factors, for example in the various types of love, reside in the habits of adjustment toward the loved object. To love a baby is to have the tendency, at least, to fondle it in a lover-like fashion. This is an abridgment of the whole behavior repertory which affords the full emotional value of sexual love. In friendship the somatic component may be reduced to a touch of the hand or a half embrace. Some facilitation of the sacral and allied mechanisms probably forms the pleasant affective core of all these experiences.¹

III. The theory which has just been advanced seems to be sustained by certain facts of genetic development which may be briefly summarized. The emotional states of the new-born babe appear to be undifferentiated. Judging of course from behavior alone they have no further character than pure unpleasant affectivity. The somatic responses are the same whether the state be caused by hunger, internal pain, or intense visual or cutaneous stimuli. The autonomic apparatus is prepared for its work at an earlier date than the

¹There is a similarity in regard to the use made of antagonistic mechanisms between the theory presented here and that of Watson ('Psychology from the Standpoint of a Behaviorist,' pp. 250-251). Positive reaction tendencies, which we may regard as a 'Watsonian euphemism' for pleasure, are said to be due to the tumescence and preparation of the sex effectors; negative reaction tendencies (unpleasantness) are ascribed to the opposite effects (detumescence, etc.) in these organs. The physiological antagonism in these two processes is obvious though not particularly stressed by Watson. The present writer believes that this theory, though correct in principle, stops short of its goal in neglecting the rôle of the remainder of the autonomic apparatus and visceral effectors. Dr. Watson also fails to bring his 'reaction tendencies' into their broader setting in the emotions.

cerebrospinal reactions which contribute the differentia of emotions. We may call this simple unpleasant experience of the new-born the 'protopathic' emotion.¹ The affective component which we have recognized as the fundamental basis of classification is thus the earliest and most primitive ingredient of human emotion. Within a short time the child gradually brings into play the various somatic responses of the pre-potent, or instinct type, such as rejecting, struggling, and withdrawing. When the afferent impulses, for example, from struggling against a blanket or an impediment to nursing are added to the protopathic core of unpleasantness we may assume that the true anger emotion begins to be felt. With overt movements of attack and defence the differentiating factor is thus added to the sympathetic pattern.

IV. A fuller comprehension of the theory may be obtained by reviewing the neural conditions for the arousal of unpleasant emotion. These conditions are necessarily those which help in breaking through the high resistance of the sympathetic and sending inhibitory impulses to the smooth muscle. (1) The first is that of the intensity of the stimulus. Almost any sensation becomes unpleasant if it is made sufficiently intense for the energy of the impulse to cross the sympathetic threshold. The peal of thunder often continues to arouse fear throughout adult life. Our theory offers a good basis for distinguishing physiologically between pains which are unpleasant and those which are not. Unpleasant pains are severe ones: their afferent impulses are powerful enough to break through into the sympathetic. The same consideration explains the pseudo-emotional quality often ascribed to intense bodily pain. (2) Repetition or insistence, such as the neural summation of petty annoyances in producing anger, is another condition favoring the arousal of unpleasant emotion. (3) Suddenness of the stimulus, or lack of proper somatic adjustment of the cerebrospinal system often causes the impulse to receive its outlet through sympathetic efferents. The fear aroused by the strange, the uncanny, or the extra-

¹ Being careful, of course, not to confuse this meaning of 'protopathic' with that now current in the English psycho-analytic school.

ordinarily large, belongs in this class. (4) Blocking of the usual somatic responses to the powerful drives, such as those of food and sex, usually through social agencies, is a potent factor in bringing about an invasion of the sympathetic. (5) Finally, the state of visceral tonus or preparation may be an important factor in lowering the sympathetic threshold and increasing emotionality. Cynicism, irritability, and other emotional attitudes indicate a permanent lowering of the resistance. Transitory effects, or moods, also increase susceptibility to fear or anger.

V. A theory of the kind we have outlined must not be considered as excluding the possibility of characteristic cortical processes in emotional and affective states. The interoceptive and proprioceptive impulses may of course be elaborated in specific ways upon reaching the thalamus or cortex. Differences in conduction such as facilitation and acceleration may also be characteristic.¹ We are here simply pointing out certain receptor and afferent processes, resulting from bodily changes, which furnish the data for emotional consciousness in the same way that the retina affords the raw materials of visual experience. The facts of behavior and physiology demand, however, that we lay due stress upon the reaction patterns which characterize both the fundamental affective component and the somatic differentia of the various emotions.

¹ For example, see Troland, L. T., 'A System for Explaining Affective Phenomena,' *J. of Abnorm. Psychol.*, February, 1920.

CONCERNING THE SENSATION QUALITY¹ A BEHAVIORISTIC ACCOUNT

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Suppose a number of people look at a patch of color—a bit of brownness on the wall. The consciousness of each of them is suffused with an immediate 'raw feel'—*brownness*. Along with this immediate brownness, there is, however, I believe, a further phenomenon. They are each at the same time set and made ready to indicate in their behavior (*i.e.*, either in words or in some more gross form of behavior) the relations of this particular brownness to other colors and, perhaps, to sensation-qualities in general. That there is this readiness, or ability, to indicate behavioristically the constitutive relations of their brownness, seems to me undeniable. For an experience of color which did not thus set them, or make it possible for them, to indicate behavioristically the relations of this color to other colors would have to be completely discounted. If a young child were entirely lacking in any capacity of discriminatory behavior towards colors, he could be assumed, for all scientific purposes, to be unconscious of those colors. Any assumption by us of such a consciousness in him would obviously be gratuitous since, by hypothesis, such a consciousness could never appear in his behavior and hence no one of us could ever know anything about it.²

Let us call this fact of the possibility of the relations of the given quality to other qualities, being indicated by

¹Read before the American Psychological Association, Princeton, New Jersey, December 28, 1921.

²See Mrs. de Laguna's very illuminating discussion in the *Jour. of Philos.*, etc., 1916, 13, 533, 617, for the view that the experience of sensation-qualities, as such, is not genetically prior in the child but develops only along with, and as a corollary of, the development of meaning and perception.

behavior, the readiness of the 'term character' of the given quality to function in behavior. That is, by the term-character of a quality let us mean the latter in so far as it receives a purely formal definition in terms of its relations of similarity to, and difference from, other qualities. And let us use the Latin word 'quale' when we wish to refer exclusively to the immediate 'raw feel,' aspect of the quality: to that aspect, namely, which, as such, fills and suffuses perceptual consciousness. We may say, then, that whenever a person senses a sensation-quality there is (1) a 'raw feel' or 'quale' directly before his mind, and there is (2), at the same time, a readiness of the corresponding 'term-character' of this 'quale' to function in his behavior.

Consider, now, the case of memory. Suppose the same people and *remember* the brown at which they have just been looking. What factors are involved in this memory? In the first place, it will be observed that the original quale as such is now no longer present. They do not, in all probability, still *see* the original brown. They may, to be sure, have a visual image of it. But, if they do, the quale of the image is not to be identified with that of the original experience. The present quale is at best but a rather dim imperfect approximation to the original quale; and they may, of course, have no visual image at all. And yet, if they have a good memory and have been at all trained in color, they will now still be as well able to describe and identify the original color as they were before. In other words the term-character of the original brown will still be ready to function in their behaviors though the original quale itself will no longer be present. In *sensation* the quale is present and the term-character is ready to function in behavior. In *memory* the original term-character is still ready to function but the quale, as such, is no longer there.¹

So much by way of preliminary orientation. Let us now attempt to bring home more clearly the nature of the be-

¹ Whatever quale there is in memory will be a different quale, namely, the quale of the image. And, it may be noted in passing, this image quale has, of course, its own term-character which can also function in behavior and which does so function in the case of introspection—when, that is, one is asked to describe his image.

haviorist's contention. It will be helpful to turn our attention for the moment to what would be the situation if we were considering not the sensation-qualities of other human beings but those of that over-worked myth, the man from Mars. The Martian, we will assume, is totally unfamiliar with the devices of human language. When we attempt to study his sensation-qualities the only method at our disposal would be one analogous to that employed by the student of animal psychology. We would, that is, use a Yerkes-Watson Discrimination Box.¹ We would present our Martian with different pairs of lights and seek to discover which lights he could be made to discriminate between in his behavior and which he could not thus be made to discriminate, behavioristically. If we carried out the procedure in a systematic fashion, we would progress down the entire spectrum taking relatively small wave-length differences and comparing each point on the spectrum so obtained with each other point. We would discover just how far apart two points would have to be in each region of the spectrum in order for a discriminable difference between them to show in behavior.

And, indeed, if we were thoroughly systematic, we would further complicate the problem and seek to discover not only which lights could be reacted to as different but also just in how far they were different. Thus, for example, we might restrict one of the stimuli to one set of regions that which we humans designate as red, orange, and violet, and allow the other of the two stimuli to range anywhere within the rest of the spectrum. If, then, our Martian could learn to discriminate in his behavior any one of the first-named stimuli from any one of the second-named stimuli, when these were paired by chance, we could conclude that the portions of the spectrum which we call red, orange, and violet are, for the Martian, all more similar to one another than any of them is similar to other portions of the spectrum; and so on. Eventually, if we arranged our problems ingeniously enough, we could discover every blessed thing about

¹ Yerkes, R. M., 'The Dancing Mouse,' Macmillan, p. 151 ff. Also, Watson, J. B., 'Behavior, An Introduction to Comparative Psychology,' Henry Holt, p. 63.

the relative similarities and differences of the different parts of the spectrum as far as the Martian's ability to react to them would be concerned. What, now, in terms of our preceding analysis, would this amount to? It would amount to our having discovered the '*term-characters*' of the Martian's color qualia. And that is all that it would amount to. We should know what parts of the spectrum he would treat as similar to one another and what parts as different from one another, and just in how far they would be similar and just in how far they would be different. But what his immediate color qualia themselves might be we would never know.

But, how, we may ask, does this situation with the Martian differ in any essential way from that involved in the observation of another human being? When I present the spectrum to a fellow man and seek to discover *his* color experiences, I do not, to be sure, require him to run about in a Discrimination Box and reward him with breaking the eighteenth amendment on one side, and punish him by giving him an electric shock on the other. But I do, nevertheless, require him to behave in a way which in all essentials amounts to just that very thing. I ask him to indicate which colors look alike to him and which different. That is, I ask him to arrange and sort the parts of the spectrum so as to indicate their relative degrees of similarity and difference. I may, to be sure, have him do this arranging and sorting primarily in verbal terms. But, even so, all that his verbal naming and pointing will indicate to me is, again, simply which colors he would treat as similar and which as different. In so far, to be sure, as he names and describes them in the same way that I should, it indicates also that his qualia and my qualia show the same similarities and differences and that we have been taught the same language. But this is all that it indicates. His behavior, whether of the gross movement type or of the abbreviated language type, can never indicate to me just what his immediate qualia, *as such*, may be. All that it shows is the nature of the term-characters of those qualia and whether or not those term-characters are the same as my term-characters. Or, stated in more precise terms, what I really

learn and all that I really learn from his behavior is, given a standardized stimulus situation (*i.e.*, a standardized physical stimulus and a standardized condition of his receptor organs and his central nervous system), to what extent his behavior *may be made* to indicate the similarities and differences of this given stimulus situation with respect to other equally standardized stimulus situations. That is, the world of stimuli presents a particular system of similarities and differences for each organism. In the case of a fellow human this system may be the same as it is for me. In the case of a visitor from Mars it may or it may not be the same. But in either case it is only this system of similarities and differences which I learn about and never the immediately experienced qualia as such. This is all that really gets into my science whether I call that science *behaviorism* or whether I call it *introspective psychology*. I, personally, prefer to call it behaviorism because that seems to me a more reasonable name for such a procedure. But the methods and problems of this behaviorism, in so far as it concerns the matter of sensation-qualities, will not be essentially different from those of introspective psychology. Only the new name will help us to be a little more self-conscious and consistent in our minds as to just what it is that we are doing.

This is all that as mere psychologists we can say. As philosophers or as metaphysicians we might perhaps attempt to go further and assert that term-character and quale are inseparable, so that to know the term-character of another organism's qualia is necessarily and *pari passu* to know his qualia. But there is a difficulty. It will be observed that in the case of memory, as we have already indicated, the term-character of the original quality may still be present and ready to function even though the quale as such is absent. Hence we know at least one case in which term-character and quale do *not* go together. The complete logical significance of this phenomenon is perhaps uncertain, but it does seem to me, judging off-hand, to strengthen the *prima facie* possibility that term-character and quale are relatively independent, and that, even though I agree with you as to

all the similarities and differences of colors, still those colors, as such, may be something for you quite different from what they are for me. If what looks green to you, looked the complementary red to me, I doubt if there would be any way in which that fact could ever be discovered. But I do not necessarily wish to stress this point. The only two things I do wish to insist upon are, first, that there is no way of proving that your qualia and my qualia are the same, and, second, that whether they are the same or not in no way affects the results of our science. All that gets into our psychology, whether we call it behaviorism or whether we call it introspectionism, is not the qualia, themselves, but merely their term characters; *i.e.*, the *behavior potentialities* of those qualia.

As a final word, let me dare to throw out a suggestion to the effect that not merely sensation-qualities but even such ultimate and recondite phenomena as awareness itself, a knowing 'me,' images, and even 'meaning' may, if they can get into psychology at all, get into a behavioristically worded psychology equally well. For (besides perhaps other additional behavior implications of some new order) these entities have, of course, their own qualia and term-characters somewhat analogous to the qualia and term-characters of sensation-qualities, and therefore they should be capable of an analogous behavioristic description and study.

THE STIMULUS-RESPONSE RELATION

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INTRODUCTION

Early in the growth of every science away from the level of mere description and tabulation it comes to the point where alternative explanations are advanced to cover the phenomena which constitute its topic. Sometimes the hypotheses are brought forward in a spirit of rivalry. Sometimes it is seen from the outset that they both have their appropriate place in the general scheme of the science, and should be assimilated together in the minds of all workers in the field. An instance of the first kind may be found in the debate for and against evolutionary conceptions which began with Lamarck and Cuvier and which has even yet hardly died out. An instance of the second kind is seen in the mutual development of Euclidean and Lobatchewskian systems in geometry.

Psychology has now reached a place where for the first time in its history this situation, characteristic of all the more highly developed sciences, confronts it. Ignoring minor distinctions we find two alternative proposals for dealing with its phenomena—Behaviorism and Introspectionism.

Now an alternative between explanations so serious and reputable that neither of them can be disregarded as obviously impossible, at once throws questions of definition, and analysis of a relational type into high relief. The business of accurately defining the hypotheses under considerations in terms of their fundamental notions assumes far more than an academic and abstract interest. For this is the very best method of determining their exact scope and mutual bearings, and eliminating unfortunate and needless conflicts. In a word, alternative hypotheses, when well and precisely ana-

lyzed, become the tools, not the stumbling blocks, of the research worker. It is for this reason that in postulate theory modern Logic has made a genuine contribution to the practical advancement of science. And Psychology, in view of the situation in which it finds itself today, may well import into its own domain some of the ideals, though not the detailed technique of the postulate method.

It will be the thesis of the following pages that the ultimate assumption of Behaviorism is in effect that all the phenomena proper to Psychology can be formulated in terms of a relation which I shall call the stimulus-response relation, such that given the stimulus, the response is determinate, and given the response the stimulus is determinate. In this very abstract statement I think we have Behavior Psychology stripped to bedrock. It will, I think, prove fruitful to consider the matter in this way, as leading to a certain precision which tends to eliminate misunderstandings and wasteful polemics.

Two very interesting discussions of the fundamentals of Psychology, by Professors Calkins¹ and Warren,² raise a number of points which can be dealt with in connection with the notion of the stimulus-response relation. Accordingly, in the twofold task of defining this relation as accurately as possible, and showing that it offers an adequate formulation of psychological phenomena, I shall have these contributions especially in mind.

RESPONSE AND STIMULUS

Our first business must be with the terms of the relation, that is, with response and stimulus.

I. What is response? Warren defines it as follows: "The activities of the effectors, together with the grosser movements and other changes which result from this activity is termed *response*."³ In a word, response is conceived of as the end-result of the functioning of a circuit from the receptors to

¹ M. W. Calkins, 'The Truly Psychological Behaviorism,' *PSYCHOL. REV.*, 1921, 28.

² H. C. Warren, 'Psychology and the Central Nervous System,' *PSYCHOL. REV.*, 1921, 28.

³ H. C. Warren, 'Human Psychology,' p. 81.

the effectors. This may be taken as a standard definition, and at the first blush seems unassailable. More detailed consideration, however, throws doubt upon its tenability and value.

In defining response as end-result Warren makes a special distinction between response and adjustment¹ by which he means the integrative activity of the central nervous system, and a more general distinction between response and the vital or vegetative processes.² Of these two distinctions, neither of which seems to me to be entirely intelligible, I will for the moment consider only the first. Are we bound to separate out adjustment from response? To put the question in another way: must we assume a three-term relation, stimulus-adjustment-response, rather than a two-term stimulus-response relation? This is no academic issue, for Warren proposes to use the concept of adjustment to reconcile the objective and subjective points of view.³

Clearly our very first question must be whether there is any reason which forces us to accept Warren's definition of response as end-result, and to separate out adjustment. If this is so, then our whole attempt to build up in terms of the stimulus-response relation collapses, for we have adopted a notion of response which involves three terms, stimulus, adjustment and response, as the ultimates for behavior.

No such reason, however, appears to exist. Warren points out that the circuit from receptors to effectors is divisible into five segments: 'receptor, sensory path, central path, motor path, effector.'⁴ Here we have definite anatomical units. Important physiological differences too exist, though these are not so well understood, and in some cases, as between motor, central and sensory tracts, are very obscure. But this is far from proof that we are bound to recognize all or some of these divisions as ultimate psychological categories. If we do so, it is merely on analogy.

It may be felt that this ignores the main point. The

¹ H. C. Warren, 'Human Psychology,' p. 86.

² H. C. Warren, 'Human Psychology,' pp. 22-23.

³ H. C. Warren, 'Psychology and the Central Nervous System,' p. 250.

⁴ H. C. Warren, 'Human Psychology,' pp. 72-73.

argument is not that the central processes are to be recognized on account of their anatomical or physiological differentiation from the others, but on account of their crucial importance in the *milieu interne*. As Sherrington says: "In the multicellular animal . . . it is nervous reaction which *par excellence* integrates it, welds it together from its components, and constitutes it from a mere collection of organs into an animal individual."¹ This is the ground for Warren's insistence on the 'neurological' method in Psychology.² Here, however, he seems to go some distance in advance of the established facts. The contention of Behaviorism would be that the action-system is probably as important as the central nervous system in determining and integrating the functions of mental life. Of course it is true that intelligent behavior on the human level would be impossible without the human nervous system. But it would be equally impossible without the human action-system. A conclusive opinion on the precise degree of relative importance to be ascribed to the nervous system and the action system in integrating behavior is as yet impossible, but it seems quite unreasonable to suppose that this work is wholly specialized to the centers. Receptors and effectors as well—indeed all the interrelated organs of the body—may be conceived as coöperating in an integrative activity, if for no other reason than that they exist together as a whole of parts within the confines of a single skin. Thus it seems clear enough that we are not *obliged* to separate out adjustment on the ground of its incontestably superior importance.

A more natural conception of response would be to take the word at its face value, and to think of it as an organic change directly contingent upon outside stimulation. If we adopt this point of view, without accepting the above as a precise definition, it is very difficult to draw the line in the receptor-effector circuit where response begins. Physiologically there are at any rate two critical points in the circuit—the point where the receptor-energies are transmuted into nervous impulse in the afferent neurones, and the point

¹ C. S. Sherrington, 'The Integrative Action of the Nervous System,' p. 2.

² H. C. Warren, 'Psychology and the Central Nervous System,' p. 268.

where nervous impulse in the efferent neurones is transmuted into effector-energies in contractile and other cells. But the functioning of each of these physiologically different segments has always one point of identity—it is always responsive in that its cause is some outside stimulation. When Warren says that the study of behavior “consists in working at the *two ends* of the nervous arc”¹ and explicitly reserves the study of the central processes as the topic for a non-behavioristic Psychology, it should be understood that he is making an assumption which may or may not be useful, but which is not made inevitable by the facts.

But this is not all. The methodological assumption involved in defining response as end-result seems to do violence to the logic of organic life which it is one of the chief merits of Behaviorism to emphasize. If we wish to deal with behavior at all, we cannot, I think, avoid regarding it as in essence the functioning of the total organism. Watson makes this the *differentia* of Psychology. “From our discussion of the scope of psychology we are now prepared to see that when the physiologist has learned all he can about the functioning of the separate organs of the body of man, he has encroached upon our field only in a very slight degree. Our task begins when the physiologist puts back the separate organs again and turns the whole (man) over to us.”² Warren too employs this concept, but does not give it the same prominence. He tells us that a man manifests behavior when “. . . he responds *as an organism* to . . . stimulation.”³

What precisely does this conception of total function mean? Certainly it cannot mean that every reaction involves the completely integrated and coördinated coöperation of every organ of the body. We may possibly believe that even with the simplest reflex there is a *tendency* for every other activity to be inhibited and for everything the organism has to become concentrated on the one reaction. But in the

¹ H. C. Warren, ‘Human Psychology,’ pp. 90–91.

² John B. Watson, ‘Psychology from the Standpoint of a Behaviorist,’ p. 20.

³ H. C. Warren, ‘Human Psychology,’ p. 11.

case of reflexes this is largely a matter of theory, and even with the highest responses it seldom or never occurs. Rather we must interpret the conception of total function as meaning that Psychology, if it is to understand human activities at all, must always interpret each overt, external act as involving not only the particular effectors thrown into activity, but the whole, integrated organism. The whole organism may not, and as a fact usually will not be drawn into active coöperation. But is only by reference to the *milieu interne* in its completeness that we can ever understand even the simplest elements of behavior.

This notion seems definitely contradictory to the view that response should be limited to end-result. Every part of the functioning of the effector-receptor circuits, it would seem, must be regarded as responsive in character. We cannot with any propriety limit the term to any one segment of these circuits if we are to talk of behavior as involving response *as an organism*. Warren tells us that "the behaviorists regard muscular and glandular phenomena as due to efferent neural activity from the centers."¹ He might also have added that they regard such phenomena as due to energies released in the receptors. Later on I shall try to show that the adjustive or integrative activity of the centers depends logically upon their responsive activity. For the moment, however, I am concerned only to point out that objective Psychology, necessarily dominated by the conception of total function, cannot but regard the whole neuro-terminal circuit as a unit. The behaviorist may, and indeed certainly does believe that the end-result is of very great importance. But a Behaviorism which proposed to build up complete explanations rigidly in terms of end-results would be absurd.

The same general line of argument which I have urged against Warren's distinction between adjustment and response can be applied to his distinction between vital functions and behavior. The contention that such processes as repair, assimilation and reproduction are outside the sphere of

¹ H. C. Warren, 'Psychology and the Central Nervous System,' p. 250.

Psychology seems based on a positive confusion of thought. It is of course possible to deal with them from the viewpoints of Physiology or Medicine, but this is equally possible with any other bodily function. And inasmuch as the so-called vital functions play an essential and immediate part in the general adaptive activity or mental life of the organism, they are relevant for Psychology. They are, moreover, responses in the ordinary meaning of the word, as following upon stimulation. We can only draw a line between behavior and vital processes by means of a very narrow definition of behavior.

By response then we must understand any bodily change whatsoever brought about by stimulation. In passing it may be worth while to note that one commonly assumed character of response cannot be accepted as universal or essential. We cannot insist that every response is, properly speaking, adaptive, though behavior as a whole must obviously be so if the organism is to survive. In general the activities of the effectors will remain the most obvious and dramatic element in response as we have conceived it. But we cannot limit our definition to end-result, or believe that any adequate study of behavior is possible that proposes to work only at the two ends of the neuro-terminal circuits.

II. I come now to my second question: What is stimulus? This will be disposed of without much difficulty. Warren defines stimulus as follows: "... the origin of every ordinary nerve impulse may be traced directly or indirectly to some force outside the nervous system acting upon the peripheral endings of the sensory neurones. This *initial* excitation is called *stimulation*."¹

The definition has the merit of avoiding what I may call the 'watershed theory' of stimulus. By this I mean the notion that we have stimulus persisting and retaining its specific difference from response up to some central point, after which it changes into response. Such a theory is not tenable for the reason that we know of no watershed, no precise dividing line where the character of impulse radically

¹ H. C. Warren, 'Human Psychology,' p. 74.

changes. The nearest approach to such a view is perhaps involved in the conception of specific energies, but this is far too debatable to help us to any decision. Then too some support is probably derived from the fact that the two terms 'sensory fibers' and 'motor fibers' suggest qualitative or modal differences which certainly have never been established. The terms 'afferent fibers' and 'efferent fibers' are thus distinctly preferable, for they suggest no more than the established fact of a vast complication of paths leading in from the receptors and out towards the effectors.

In one respect, however, Warren's definition seems open to question. He would regard the energies released in the receptor as constituting the stimulus. In the first place, however, the work of integration unquestionably begins in the sense organ. For instance when the impulse is transmitted from the rods and cones to the optic nerve it is no longer a crude physical energy, but has already been in large measure assimilated to and subsumed by the organic set-up. And secondly, for a Psychology which operates in terms of total function, the critical point is not, as it is for Physiology, between receptor and afferent neurone. That is, it is not at the point where the influence becomes a nervous impulse. Rather it is at the dividing line between the organism and the outside world. This is where the change that is most deeply significant for Psychology actually takes place. And accordingly we must not say that the stimulus is constituted by the energies of the receptor, but rather by those outside physical influences which throw the receptors into function.

We must, however, be a little careful about the use of the word 'external' as applied to stimuli. What about intra-organic stimuli? For instance the speeding up of the heart beat is likely to be a part of many response patterns, and at the same time it may become the stimulus which leads me to take a drink of water or throw open the window. This is not a point that need be taken very seriously, and I mention it only to dispose of a possible objection. The actual physical influence released by the speeding up of the heart beat is as truly 'external' to the make-up of the organism as the light

waves which impinge upon the retina. If I kick myself, the act of kicking is one of my responses, but the impact is just as 'external' as if I had hit myself with a hammer.

A stimulus then is any external influence which modifies the condition or functioning of an organism, or, adopting the definition already worked out, any influence which brings about a response. This is a very wide characterization, and may be objected to on that score. But once we adopt the notion of total function we obliterate minor distinctions, and I do not see how such a definition of stimulus can be avoided.

The ultimate justification of any concept is of course its value for clearing up scientific procedures. I shall have more to say about this in the following section. For the moment however, without anticipating unduly, I may pause to point out that inclusive as such a definition undoubtedly is, it is not so difficult to accept as may appear at first sight.

Perhaps the most serious objection to defining stimulus as any external influence which modifies organic condition is that it includes such things as ultra-violet rays, drugs, the metabolisms of old age and so forth. These are precisely the sort of agents that Psychology does *not* usually regard as stimuli. I believe, however, that reflexion will show that the only adequate reason for refusing to regard them as stimuli is that they are not correlated to any clear-cut, dramatic, unitary responses. We tend to think of a response as a definite movement quickly over, and the effects of exposure to ultra-violet rays, of a dosage of some drug, or of excess of katabolic processes are not in this class at all. But they most assuredly do register in behavior. Their effects take the form of modifications of the curve or work, of partial or complete breakdowns in motor coördinations, of slowing of reaction-times and so on. And must not such variables as these be classed as responses? The effects of a five-grain dose of caffeine are not so picturesque as those of a sharp electric shock. But they occur in the same medium, namely total organic function, and are both studied by procedures whose essential logic is identical.

To sum up, a response is any change whatsoever in the condition of an organism, brought about by external influence.

And a stimulus is any external influence which brings about a response. Defining our concepts in this way we posit a two-term stimulus-response relation such that given the stimulus the response is determinate, and given the response the stimulus is determinate.

THE STIMULUS-RESPONSE RELATION

The way is now clear for the central question of this discussion: Have we in the stimulus-response relation as conceived, a generalization adequate to the facts of human behavior? The most obvious difficulty in the way of such an assumption seems to be the wide variability of human actions. In view of this it may appear almost meaningless to think of response as a constant function of stimulus.

1. A stimulus may give rise to a response that is immediate and adequate. By an immediate response I mean one that follows on its stimulus after a normal reaction-time interval. By an adequate response I mean one that deals adequately with its stimulus—that puts the organism in possession of the stimulus, to use the classic phrase.

2. A stimulus may give rise to a response that is not wholly adequate. An insect crawling on a sleeper's face will give rise to more or less ill-adjusted twitchings, whereas in an adequate response these would be coördinated with movements of the arms and hands and perhaps of the head and trunk, which would get rid of the irritation. What Watson calls implicit responses¹ mostly belong in this class, though not all of them do. In particular, while a suppressed speech-reaction may under certain circumstances be perfectly adequate, putting the organism in possession of the stimulus, it may under other circumstances be very inadequate. Cursing under one's breath is not always a satisfactory substitute for smashing something.

3. There is a large and important, but on the whole more or less neglected group of deferred responses. The delay between stimulus and response may be quite brief, as with the ordinary long reaction-time in free association. Or it may be protracted indefinitely, as would be the case with

¹ John B. Watson, 'Psychology from the Standpoint of a Behaviorist,' cf. p. 14.

dreams. In this latter case, according to Freud, the delay would rarely be more than twenty-four hours, but according to Jung and the Zurich school very much longer. Remembering also would have to be classed as deferred response.

4. The response to a given stimulus may be modified almost indefinitely with time and circumstance. Most characteristically this is the work of habit.

In the first place it should be clear that this extreme and impressive variability does not necessarily involve any sort of indeterminism. To take an example from Physics, let us suppose that two moving bodies *A* and *B* collide. It would be absurd to suppose that the momentum of *A* has no calculable relation to the resultant momentum merely because the velocity of the two bodies together is quite different from that of *A* by itself. In the same way it would be absurd to suppose that a stimulus *X* has no determinate relation to behavior because it has one effect when the individual is doing nothing in particular except perhaps awaiting *X*, and quite another when the individual is busy with a task. We may assume that if the organism were always perfectly receptive, if we could translate Locke's *tabula rasa* doctrine into physical terms, repetitions of the same stimulus would always issue in the same result, and that the result would be an immediate and adequate response. And while this ideal situation never occurs, we are not thereby forbidden to think of a determinate relation between stimulus and response.

But the variability of behavior raises a much more serious objection. We may admit that it does not involve indeterminism. But it may seem to involve a complexity so great that the notion of a stimulus-response relation as ultimate will prove to be inadequate. Warren would insist upon this difficulty, and say that we must introduce adjustment as a third term in order to explain the facts. "A balanced view of the field of psychology then would seem to make its central feature the *specific activity of the nervous system*."¹ In particular the concept of adjustment may seem essential if we are to give any account of habit-formation. Let us examine this contention.

¹ H. C. Warren, 'Psychology and the Central Nervous System,' p. 250.

A. Explanation in terms of adjustment may mean using some highly abstract brain-scheme such as that proposed by Meyer.¹ Without denying that such ingenuities may have some pedagogic value, I cannot see that they really explain anything. They suggest a schematic completeness which our knowledge very conspicuously lacks, and seem to offer a case not so much of *ignotum per ignotius* as of formulating partially known facts in terms of neat guesses. To cite Watson on the point: "We need in psychology all the available facts that the *neurologists* can give us, but we can very well leave out those ingenious puzzle pictures that compare the action of the central nervous system with a series of pipes and valves, sponges, electric switchboards and the like."²

B. Explanation in terms of adjustment may mean that we must take into account the functioning of the central nervous system as it actually goes on. Surely however this is a truism. No behaviorist, I presume, would refuse to do this. He may believe that end-result is important, but it is no exclusive obsession. Warren remarks: "Behaviorists are inclined to emphasize the adjustive effects of the motor processes on the total response;"³ and implies that the adjustive effects of the central nervous system are far more important. So far as this is a mere question of fact, no general principle of importance seems to be raised.

But it will be said that here we have more than a question of fact, for the integrative action of the nervous system appears entirely different from and logically prior to response. If this is indeed the case then explanation in terms of adjustment will be more ultimate than explanation in terms of response, and we may look for the sort of synthesis which Warren suggests in adopting the neurological point of view. Now it is true that we can regard effector activities as controlled by central processes of integration and conduction. But surely this is the central concept not of Psychology but of Neurology. For Psychology the primary consideration is

¹ Max Meyer, 'The Fundamentals of Human Behavior.'

² John B. Watson, *op. cit.*, p. 19.

³ H. C. Warren, 'Psychology and the Central Nervous System,' p. 267.

not that the individual possesses a nervous system, but that he responds to environmental stimulus *as an organism*. But does not this depend upon the central functions? I do not think we can say so. Rather the dependence seems to work the other way. I have already pointed out that every part of the receptor-effector circuits shares in the work of integration. The question I now raise is: What is integration, or rather, what is it that is integrated? It can be nothing other than the effects of stimuli, that is, responses. An impulse is released in the organism by external stimulation. The organism itself is a highly intricate machine, so the effect produced by the new impulse is far from simple. It comes at once into relations with a mass of functions already going on, and is woven into their texture. This is adjustment, and as so conceived it is not logically prior to but logically dependent upon response to stimulus. Just as physical equilibrium is nothing in itself, and means only a balancing of many forces, so adjustment is nothing but the intercalation of responsive activities which is inevitable when stimuli impinge upon a highly complex mechanism like the body.

Professors Warren and Calkins both urge that the thoroughgoing formulation of mental life or adaptive behavior in terms of response breaks down when we come to the thought processes. In particular the doctrine that thinking is implicit language-reaction is criticized in the interests on the one hand of a Psychology of adjustment and on the other of a Psychology of the self. Let us review the contentions they advance.

A. It is said that the same thought can be expressed in many different ways—that is, in many different word-forms, and that there must be some psychological expression of its identity in all these differences. Professor Calkins writes: "To take Watson's own example from geometry. On his view the solution of a proposition consists essentially of a series of laryngeal reactions. But the identical solution of the same problem involves wholly different laryngeal reactions if the mathematicians talk different languages—if one, for example speaks of *Winkel* while the other talks about angles."¹

¹ M. W. Calkins, 'The Truly Psychological Behaviorism,' p. 7.

Warren also argues in the same sense.¹ This point turns on what we mean by saying that a chain of reasoning is identical although expressed in different media. To say that a proposition remains identical whether expressed in English or German, or, what is logically more important, whether expressed in terms of one or the other of alternative sets of postulates, means only that a systematic equivalence can be established between the two sets of symbols. And all that we need for this is some formula which will transform the one into the other. That is to say, the identity can be explained on wholly external grounds of a formal character. And this implies nothing at all about the self or the nervous system. We have no need to seek some deep and hidden identity, for we can get along without it.

B. Warren urges that the description of thought as language reaction involves what he calls the 'thermometer fallacy.'² Just as the temperature of the room is not identical with the movements of the mercury column in the thermometer, so thought is not identical with laryngeal work. This is perfectly true if we grant his definition of response as end-result. But the behaviorist need not do so. It will be enough for him if the stimulus issues in some change in organic condition, even though this is not transmitted as far as the effectors. For him this will be the response in terms of which to explain the process. Thinking will usually manifest itself dramatically as laryngeal innervation. But the behaviorist need suffer no fundamental disconcertion if the impulse sometimes does not reach so far. His explanation can still retain its essentially objective character, and the function in terms of which it will be made will still be a function of response.

Thus we must conclude that in the stimulus-response relation we have a concept that is not inadequate to serve as the basis for an objective Psychology.

THE STIMULUS-RESPONSE RELATION AND CONSCIOUSNESS

The question now arises as to what we are to do with

¹ H. C. Warren, 'Psychology and the Central Nervous System,' p. 260.

² H. C. Warren, 'Psychology and the Central Nervous System,' p. 258.

consciousness. In view of our analysis of the stimulus-response relation, we cannot accept Warren's reconciliation of the subjective and objective points of view. He writes: "Introspective psychology studies the *receptive* phase of the process—awareness of the environment. Behavior psychology studies the responsive side—the creature's reactions upon the environment."¹ And his solution is to concentrate upon the intermediary process of central adjustment. But our discussion has gone to show that the antinomy between the receptive and the responsive processes does not exist. For us the creature's awareness of the world is not an affair of passive receptivity, but of positive response to stimuli.

Shall we then say that all we have is organic reaction to stimulation, and that consciousness in the traditional sense does not exist at all? While this is the unmistakable tendency of behavior Psychology, I do not think that such a conclusion is legitimate. At the same time the logic of Behaviorism is invaluable in helping us to rule out certain theories of the nature and relations of consciousness. Let us pause to consider these.

1. The whole tenor of our discussion of the stimulus-response relation goes to show that epiphenomenalism is quite untenable. The systematic correlation of psychoses with neuroses presupposes very simple unitary neuroses, which in fact are not found. Quite apart from the difficulty of conceiving of a constant parallelism without any interaction, this theory is based upon a misconception of the nature of nervous function. And its impossibility becomes still more apparent if we admit that the work of integration is not confined to the nervous system.

2. Again we are led decisively to reject the idea that consciousness somehow intervenes between stimulus and response. If a self-consistent and adequate Psychology can be erected on the basis of the stimulus-response relation, then such a notion is not needed. Furthermore, as Thorndike has shown, the alleged mediation of ideas is in many cases not effective. As he points out,² an idea will lead to a response

¹ H. C. Warren, 'Psychology and the Central Nervous System,' p. 267.

² E. L. Thorndike, 'Ideomotor Action,' *PSYCHOL. REV.*, 1913.

that in any way corresponds to it, often only after a long process of learning and sophistication.

3. We cannot derive any decisive defence of consciousness from an appeal to mnemonic phenomena in the sense intended by Semon.¹ There seems no difficulty in explaining these phenomena on physical, or rather organic grounds. It may seem that in order to deal with them we need the concept of habit, which in turn depends on the idea of the forming of neural paths, and that this leads to the separating out of adjustment. The reply however is that we have already shown the whole complexity of the central processes to be logically dependent upon response. Thus we have nothing here to invalidate our conception of the stimulus-response relation.

What then is suggested as the correct view of the nature and relations of consciousness?

The reason why a behavior Psychology which proposes to deal with its phenomena in terms of the stimulus-response relation must not dogmatically rule out consciousness is that the stimulus-response relation itself is nothing more than a working hypothesis, a postulate. Now what we ask of a working hypothesis is that it shall be self-consistent, adequate, and serviceable for research. All of these conditions are fulfilled, it seems to me, by the stimulus-response relation. But we must never suppose that a working hypothesis rules out all other ways of dealing with the phenomena which it handles from one special point of view. Most phenomena indeed are quite susceptible of several formulations. And this seems to be the situation here. The fact that we may have a consistent Psychology which explains in terms of a hypothetical stimulus-response relation is no reason why we cannot have a consistent Psychology which explains in terms of a hypothetical consciousness.

To be specific, the notion of a stimulus-response relation does not necessarily rule out Professor Calkins' ideal of a self Psychology. In 'self psychology' she sees the science "of (a) a behavioristically conceived individual, the self, in (b) behavioristically conceived relations to (c) a behavioristically

¹ R. Semon, 'Die Mneme.'

described environment.”¹ There appears no reason why all the relevant phenomena cannot be treated either along these lines or in terms of the stimulus-response relation. In both cases we have formulations that are self-consistent and adequate in the sense of applying to every part of the field. We can always secure a transformation from one to the other set of terms, and the whole question of preference reduces to one of practical utility for research. Such a situation is perfectly familiar in other sciences, and we shall have to produce excellent reasons before ruling it out in Psychology, especially as it has the great advantage of definitely shelving all metaphysics and all dogmatism.

This of course calls for a broad conception of the range of Behaviorism. We cannot be wholly content with Watson's rather cavalier rejection of “such terms as sensation, perception, attention, will, image and the like” with the comment, “I frankly do not know what they mean, nor do I believe that anyone else can use them consistently.”² Whether these particular terms are used or not is a small matter. But it is important to see that behavior Psychology has not fulfilled itself until it has found their equivalents in terms of stimulus and response.

To sum up, much of the polemics between behaviorists and introspectionists seem like beating the air. They can, I think, be constructively avoided by analyzing down to fundamentals. For they arise from a misunderstanding, a failure to see that whichever point of view we adopt, we do not *ipso facto* deny the possibility of the other. Let it be recognized that both behavior Psychology and self Psychology are in the last analysis working hypotheses, ideals of procedure, and there seems nothing to prevent their proponents from living in as much coöperative harmony as euclidean and non-euclidean geometers, who do not fill the air with debate about the truth of the parallel postulate.

¹ M. W. Calkins, ‘The Truly Psychological Behaviorism’. *Psychol. Rev.*, 1921, 28, 14.

² John B. Watson, *Op. cit.*, p. viii.